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XXVIII. *On the Ascent of Hollow Glass Bulbs in Liquids.*
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1. WHEN a hollow glass bulb is detained at some depth below the surface of a liquid and then released from its position, it rapidly acquires a sensibly uniform velocity; and the motion continues to be characterized by such velocity until the surface is reached or nearly reached, the velocity at that time being much accelerated. The bulb next rises somewhat out of the liquid and then falls; oscillating thus, until it finally comes to rest under the usual conditions of a floating body. In the present paper I propose to give an account of an experimental investigation which has reference to the stage of uniform velocity.

2. The bulbs I have generally employed consist of Fig. 1.
lead glass, and have, as will be seen in the accompanying figure, a somewhat pyriform shape; the stalk terminates in a hook, which carries an adjusted glass weight. A few small projections are sealed on to the body of the bulb in order to prevent too close accidental contact with a wetted boundary. In working with these bulbs, I have always effected the depression with a piece of glass tubing, and find that



Scale $\frac{1}{2}$.

* Read May 14, 1881.

after some practice, it is seldom an experiment is lost by lateral divagations of the bulb. It is necessary that all the surfaces be clean.

I. *Change of the Diameter of the Reservoir.*

3. A series of glass cylinders having been selected, their mean diameters between two fixed marks 220 centimetres apart were determined. These marks were ruled at places sufficiently distant from the surface and bottom of the liquid to ensure the uniformity of the motion observed between them. The liquids were first heated a little above the temperature desired for the experiments, then poured into the cylinders; and these were immersed in a much larger glass vessel containing water of very nearly the same temperature. The thermometers used could be read directly to tenths, and to hundredths by estimation. The times were taken with a metronome beating thirds of seconds.

4. The following experiments were made at 16° with a mixture of alcohol and water of sp. gr. .9120 at that temperature; they are compared in the table with numbers calculated from the equation

$$\left. \begin{aligned} (t - \tau)d^2 &= c, \\ (t - 6.10)d^2 &= 23.59. \end{aligned} \right\}$$

TABLE I.

<i>d.</i>	<i>t</i> obs.	<i>t</i> calc.
5.66	6.56	6.84
4.76	6.83	7.18
3.37	8.17	8.18
2.79	8.62	9.13
2.20	12.08	10.97

Probable error of a single experiment, 0.44.

The experiments were actually made on a height of 11 centim.; and the values obtained for *t* were half those given in the table. Each result is a mean of seven determinations.

From the above comparison, it appears that the product of the number of seconds *t* (less a constant τ) into the square of the diameter *d* of the cylinder is a constant quantity *c*. The time of ascent through a cylinder of infinite diameter would have been $\tau = 6.10$ seconds. The law, however, shows indications of failure when the diameter is narrowed to 2.2 centim.

5. Table II. contains the results of some experiments with recently-boiled water, at 15°, and another bulb. The time is calculated to seconds. Each result is a mean of these determinations.

TABLE II.

<i>d.</i>	<i>t</i> obs.	<i>t</i> calc.
17.16	26.2	26.17
9.24	26.1	26.71
5.66	26.3	27.97
4.76	27.7	28.80
3.37	29.9	31.63
2.79	37.6	34.24
2.20	41.0	39.20

$$\text{Equation, } (t - 25.95)d^2 = 64.56.$$

Probable error of a single experiment 1.29.

Sum of the errors (*t* obs. - *t* calc.), +0.08.

The last two numbers suggest the same remark as in (4).

6. A sample of hydrocarbon-oil was next tried, a different weight and bulb being used. Each result is a mean of six determinations. The sp. gr. of the oil was .9120 at 16°, *i. e.* the same as that of the mixture of alcohol and water referred to in (4).

TABLE III.

<i>d.</i>	<i>t</i> obs.	<i>t</i> calc.
5.66	31.5	31.41
4.76	35.3	34.91
3.37	46.0	46.80
2.79	57.4	57.73
[2.20	90.8	78.88]

$$\text{Equation } (t - 22.96)d^2 = 270.65.$$

Probable error of a single (1-4) experiment, 0.37.

II. *Change of the Unbalanced Pressure.*

7. The unbalanced pressure, to which the ascent of the bulb is due, can be conveniently changed by increasing or diminishing the weight attached to the bulb. If we take a centimetre as the unit of volume, and understand by δ the mean density of (bulb + contents + weight) referred to that of the

liquid as unity, then $(1-\delta)=p$ may be termed the "unbalanced unit pressure."

The following experiments were performed with a bulb of the usual size, recently-boiled water at $11^{\circ}5$ being the liquid. The diameter of the cylinder was 5·66 centim. ; the times are stated in half-seconds; and each result is the mean of seven determinations.

TABLE IV.

<i>t.</i>	<i>p.</i>	<i>tp².</i>
20	·01139	·0025946
40	·00810	·0026244
81	·00569	·0026225

Equation, $tp^2=c=·0026138$.

Probable error of c , 0·25 per cent.

8. Another set of experiments was carried out in the same cylinder, under nearly the same conditions, with five variations of weight. Each result is the mean of five observations of ascent through a height of 16·5 centim. The times are stated in thirds of seconds.

TABLE V.

<i>t.</i>	<i>p.</i>	<i>tp².</i>
15·5	·00438	·00029735
31·0	·00310	·00029791
60·0	·00223	·00029844
97·0	·00177	·00029705
146·2	·00143	·00029806

Equation, $tp^2=c=·00029776$.

Probable error of $c=·000000169=0·06$ per cent.

The product of the square of the unbalanced unit pressure into the time is therefore a practically constant quantity.

III. *Change of Content.*

9. In all the experiments above recorded the bulb contained air, probably at a pressure slightly below that of the atmosphere. The mean density of (bulb + contents + weight) is of course affected by the contents of the bulb, and then also the rate of ascent.

If we regard as correct the mathematical relation last

arrived at (8), it is easy to deduce from it an equation which involves the specific gravity of the contained gas. Assuming other conditions to remain unchanged, the density of the gas is

$$\gamma = A + B \sqrt{\frac{t - \theta}{t}}.$$

In this expression t is the time of ascent through the constant height for any particular gas, θ the time of ascent corresponding to a gas of density A , and B the density of a gaseous content which just fails to let the bulb sink. It is convenient to put $A=0$, which corresponds to the bulb being vacuous ; B and θ are then deduced, as the "constants of the bulb," from two independent determinations.

10. In order to change at pleasure the contents of the bulb its form was slightly altered, as shown in fig. 2, and its volume enlarged. The two stoppers with which it was furnished were coated, on their ground surfaces, with a scarcely perceptible film of grease; the operator's hands were encased in gloves ; and every experiment was carried out as speedily as possible. The gases were employed in the dry state, at an average barometric pressure of about 75.7 centim.; in the following table their densities are referred to hydrogen. Each result is a mean of several determinations; and the times are given in thirds of seconds.

Fig. 2.



Scale 1.

TABLE VI.

Gas.	γ found.	γ .	t .
Carbonic dioxide ...	21.81	22.00	72.5
Oxygen	17.17	16.00	38.2
Air	14.13	14.50	30.6
Water-gas	6.44	6.00	23.0

The equation is $\gamma = 26.032 \sqrt{\frac{t - 21.589}{t}}$.

Probable error of a single comparison, 0.51.

Great accuracy in work of this kind is, of course, out of the question ; but for approximate determinations, when only very small volumes of a gas are available, the method will probably be advantageous.

11. The following set of experiments has reference to a medium consisting of two mixed liquids, the rate of ascent of a bulb in each of which separately is known. One of the liquids was castor-oil, the viscosity of which is very considerable; to 100 parts of this, successive quantities z of linseed-oil were added so as to gradually reduce the viscosity. The uniform temperature was $12^{\circ}4$. By separate experiments with the oils it was found that, if t were the rate of ascent of the bulb in seconds per centimetre, λ the rate for linseed, α the unbalanced unit pressure in castor, and β the change in that pressure for every unit of linseed added, then

$$\begin{aligned} (t - \lambda)(\alpha + \beta z)^2 &= c, \\ (t - 32335)(0.09122 + 0.00169z)^2 &= 0.034376, \end{aligned} \quad \left. \right\}$$

are equations which fairly represent the experiments.

TABLE VII.

t .	z taken.	z calc.
4.45454	0.0	0.0
3.10027	11.1	11.9
2.21798	25.0	25.7
1.52758	42.9	46.0
1.22140	66.7	61.8
0.32335	∞	∞

Probable error of a single comparison, 1.7.

Sum of the errors, +0.3.

12. Castor-oil presents the remarkable property of recovering its normal viscosity very slowly after heating. A sample of the oil, through which a bulb rose in a known number of seconds per 22 centim., was heated to a temperature not exceeding 80° , and cooled to its previous condition. The bulb now took ten seconds less for its ascent. After a day's repose it rose again in the oil with its original velocity.

13. The fundamental formulæ given in this paper admit of easy extension to other cases. I have, for example, found in the ascent of a hollow glass bulb a good indication of the maximum density of water, and of the amount of dissolved matter in saline solutions; and it could obviously be applied to determine the specific gravity of solid bodies.

The experiments which I have recorded show clearly that

the rate of ascent of a solid through a liquid depends, not only upon the density of the liquid, but upon some other property—probably the viscosity. When the ascending solid approximates in diameter to the diameter of the reservoir, special retardation occurs, and some new law controls the motion. The commencement of this retardation is earlier with oils than with water, earlier with water than with alcohol and water—earlier, in short, when viscosity is greater. In reservoirs of sufficient diameter, the rate is inversely proportional to the square of the altered condition.

My thanks are due to Messrs. Ellis and Smith for their aid in this investigation.

Glasgow, March 1881.

**XXIX. *Change of State: Solid-Liquid.* By J. H. POYNTING,
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[Plate XIII.]

Two distinct types of change of state from solid to liquid have usually been recognized. The most familiar of these is the ice-water type, in which, as the temperature rises, the solid remains quite solid up to the melting-point; when this is reached it begins to melt *at the surface*, and the temperature remains constant till the whole is liquid, when the temperature again rises. Corresponding to this change of state there is a definite latent heat. In the second class of bodies, of which sealing-wax and phosphorus are examples, there is a gradual softening as the temperature rises; and this softening takes place *throughout the mass*. There is no definite arrest of the rise of temperature, and no definite latent heat.

It has sometimes been supposed that the ice-water type is merely a limiting case of the sealing-wax type, where the softening takes place, but through a very small range of temperature. Prof. Forbes held this view, and by it attempted to explain regelation; but subsequent experiments have not supported the theory, and I believe it is now generally abandoned.

Since, in the ice-water form of change of state, fusion only

takes place at the surface, it seems much more probable that it is an exchange phenomenon analogous to the change which takes place when water is evaporating, according to the kinetic theory. Just as in the case of water-steam, a steady state is reached when the number of molecules escaping from the surface of the water into the gas is equal to the number passing from the gas into the water, so in the case of water-ice a steady state (that is to say, the melting-point of ice) is probably reached when the number of molecules passing from the ice into the water is equal to the number passing from the water to the ice. For the analogue of the sealing-wax type of melting we must probably take the change of state which takes place in a liquid-gas above its critical point, where it changes gradually from a state rather liquid than gaseous to a state certainly gaseous.

In this paper I shall attempt to support this view of solid-liquid change of state. The following is a summary of the argument and the conclusions arrived at.

It is assumed that the maximum vapour-tension of a substance at any temperature is an indication of the number of molecules crossing its surface in a condition to escape. Now Regnault's experiments show that at 0° ice and water have the same vapour-tension; that is, the number of molecules crossing the surface of the ice ready to escape is equal to the number crossing the surface of the water in the same condition. Hence, when the two are in contact at 0° , the interchange of molecules is equal. For temperatures below 0° , Kirchhoff has shown that the vapour-tension of water is greater than that of ice, and above 0° it is less than that of ice—if ice can exist. (Another proof of this theorem is here given.) It is, then, easy to give a general explanation of the phenomena of melting and freezing by supposing that, if the temperature is not at the melting-point, the substance in the state with the greater vapour-tension will lose at the expense of the state with the less vapour-tension.

To explain the alteration of the melting-point by pressure, we must suppose that pressure alters the vapour-tension, and therefore the rate of escape of molecules, and that this alteration is different for the two states. Sir William Thomson has shown that a liquid in a capillary tube is in equilibrium with

its vapour at a greater or less tension than at the plane surface according as the surface is convex or concave, upwards, and has given a formula for the difference. Accompanying this curvature of surface is a difference of pressure in the liquid; and I suppose the variation of vapour-tension to be due to the difference of pressure. A proof is given of Sir W. Thomson's formula, which seems to bring out more clearly the connexion of the phenomenon with the pressure, and which seems to apply to solids as well as liquids. According to this formula, the steady state (the melting-point) may be reached at any temperature if the pressure can be so adjusted that the vapour-tensions in the two states at that temperature and pressure are equal. The resulting lowering of the melting-point by pressure agrees in amount with that given by the well-known formula of Prof. J. Thomson.

It follows from this mode of regarding the subject, that, if in any way the ice can be subjected to pressure while the water in contact with it is not so subjected, then the lowering of the melting-point per atmosphere is about $11\frac{1}{2}$ times as great as when both are compressed. I give the results of some experiments which I have made to test this, and which certainly seem to indicate that the fall of melting-point is much greater than the amount usually supposed if the ice alone be compressed.

The isothermals for ice-water are then discussed. It has been supposed that, if we could employ a sufficiently low temperature and high pressure, then ice would pass continuously into water; that is, the isothermals would have no horizontal part corresponding to a mixture of ice and water, and we should have a critical point. Assuming, however, that a mixture of ice and water completely freed from foreign gases can be subjected to great negative pressure or tension, it seems probable that there is another critical point at a temperature above 0° and at a high negative pressure; that is, the water-ice line is a closed curve. We know that below 0° the water isothermals can be prolonged below the horizontal portion, since water is unfrozen in certain cases,—and that the ice isothermals can be prolonged above the horizontal portion; for ice, at 0° say, can be suddenly compressed without melting in the interior. This suggests that the true form of the isothermals

is a continuous curve, of the nature which Prof. J. Thomson has suggested in the case of liquids and their vapours.

If we suppose that the curves are continuous in the same manner for ice-water above 0° , then Prof. Carnelley's "Hot Ice" would seem to be represented by the prolongations upwards of the ice isothermals beyond the horizontal line to where they meet the line of no pressure. The critical point, which certain assumptions roughly fix at about 14° C., would then be an upper limit, or rather above the limit, to the temperature of hot ice in a vacuum.

In conclusion, it is pointed out that the sealing-wax type of melting is probably similar to the change of ice into water below the lower, or above the upper, critical points, if these exist.

Melting and Freezing of the Ice-water Type at ordinary Temperatures and Pressures.

It seems to have been conclusively proved by experiment that, in bodies of the ice-water type, change of state, either from solid to liquid, or the reverse, takes place only at the surface, or at a surface separating dissimilar portions. This would also seem to follow from the fact that the change of state always requires a certain finite amount of energy to be abstracted from, or supplied to, the mass without alteration of temperature. In the middle of a homogeneous body, where the temperature varies gradually, we must have the energy per unit of volume a continuous quantity as we pass from point to point. Hence, when at any point there is sufficient energy per unit of volume to change the state, either the surrounding temperature must be far above the ordinary temperature for change of state, or the surrounding substance must occupy an intermediate condition between the two states. On the former supposition we should certainly not have the ordinary change of state, though something of the sort may occur in the case of Dr. Carnelley's "hot ice;" and in the latter we should have the sealing-wax type, and no signs of this have been observed.

Since, then, change of state is a surface phenomenon, we are led at once to connect it with the escape of molecules which we know to be always taking place from the surface, as indicated by the definite vapour-tension which the body possesses,

whether solid or liquid. Now Regnault's experiments have shown that at 0° ice and water have the same vapour-tension, and at the same time a mixture of ice and water at that temperature maintains the same proportion between the two constituents as long as no heat is allowed to pass into or out of it; that is, as many molecules escape from the water into the ice as pass in the opposite direction from the ice into the water. We seem, then, to be justified in assuming that *the number of molecules coming up to a given surface with a sufficient velocity to escape is indicated by the maximum vapour-pressure at that temperature.*

Now suppose that we have a mixture of ice and water below 0° . Kirchhoff has shown (Pogg. Ann. ciii. p. 206) that below 0° the vapour-tension of water exceeds that of ice by .044 millim. of mercury per degree; and his reasoning will equally prove that it falls below it by the same amount above 0° , if ice can exist at such a temperature. Prof. J. Thomson has subsequently (Brit. Assoc. Report, 1872, p. 24; Proc. Roy. Soc. 1873; 'Nature,' ix. p. 392) arrived at a similar conclusion independently. A proof differing in arrangement from Kirchhoff's, and following out rather the line indicated by Thomson, will be given below.

In a mixture, then, of ice and water below 0° , since the water has the greater vapour-tension, more molecules will cross the surface from the water to the ice than in the opposite direction. The ice will therefore gain, while the water loses. At the same time the molecules will possess less energy when arranged as ice. Hence the temperature of the whole will rise, and this rise will go on till 0° is reached, when there is once more equilibrium—or till the whole is converted to ice, if that condition be previously reached. This seems sufficiently to explain the action of a small piece of ice dropped into water below 0° ; and the fact that the change of state is a surface phenomenon seems to show that the presence of some ice is necessary to commence change of state.

If a mixture of ice and water at 0° be supplied with heat, as soon as the temperature rises ever so little above 0° the equilibrium of exchange is destroyed; for the vapour-tension of ice becomes greater than that of water, and therefore the number of molecules entering the water from the ice is greater

than the number going in the opposite direction. But since the water arrangement requires more energy, heat is absorbed, and the mixture has a tendency to fall back to 0° .

Before going on to discuss the effect of pressure on the melting-point, I give a proof, with a somewhat more general result, of Kirchhoff's formula,

$$\frac{d\varpi'}{dt} - \frac{d\varpi}{dt} = .044 \text{ millim. of mercury,}$$

where ϖ' is the maximum vapour-tension of ice, and ϖ that of water.

Start with a volume v of water at temperature $-t^\circ$. Let it evaporate, always at the temperature $-t^\circ$, in a cylinder which it does not wet, at its maximum vapour-tension ϖ , which we suppose to be maintained by a piston. Let the ultimate volume of the water-vapour be V . Then the external work done in the expansion is $\varpi(V-v)$.

Now let the vapour further expand, always at the same temperature and in equilibrium with the pressure, till we have reached a volume V' at the maximum vapour-tension ϖ' of ice. Assuming Boyle's law to hold, the work done in this expansion is $\varpi'V' \log \frac{\varpi}{\varpi'}$; and this would be 0 if $\varpi=\varpi'$.

Now introduce a particle of ice at $-t^\circ$ into the cylinder, and condensation into ice will go on till all the vapour has disappeared. If the ultimate volume of the ice is v' , the work done on the substance is $\varpi'(V'-v')$.

Increase the pressure from ϖ' to $\varpi'+p$ till the melting-point is lowered to $-t^\circ$. If κ' is the coefficient of cubic compressibility of ice, $\frac{p^2}{2}\kappa'v'$ is the work done in the compression.

Introducing a drop of water, allow the whole to melt into water under the pressure $\varpi'+p$, the work done during the melting being

$$(\varpi'+p)\{v'(1-p\kappa')-v(1-p\kappa)\},$$

where κ is the coefficient of cubic compressibility for water.

Now let the water expand to its original volume v by gradually reducing the pressure to ϖ . The external work done is $\frac{p^2}{2}\kappa v$.

We now have the substance in its original state; and the cycle through which it has been taken was reversible at every step; therefore

$$\int \frac{dQ}{T} = 0.$$

But T is constant; therefore

$$\int dQ = 0.$$

Then the total external work is zero, or

$$\begin{aligned} \varpi(V-v) + \varpi'V' \log \frac{\varpi}{\varpi'} - \varpi'(V'-v') \\ - (\varpi' + p) \left\{ v' \left(1 - \frac{p\kappa'}{2} \right) - v \left(1 - \frac{p\kappa}{2} \right) \right\} = 0. \end{aligned}$$

By means of the equation

$$\varpi V = \varpi' V',$$

and neglecting products of ϖ and κ , this reduces to

$$\varpi V \log \frac{\varpi}{\varpi'} = p \left\{ v' \left(1 - \frac{p\kappa'}{2} \right) - v \left(1 - \frac{p\kappa}{2} \right) \right\} + (\omega - \omega')v. \quad (1)$$

Neglecting the term $(\omega - \omega')v$, and putting for $\varpi'V$ $\omega_0 V_0 \alpha T$, where $\omega_0 V_0$ are the pressure and volume at 0° C., and T the absolute temperature, we have

$$\frac{\varpi}{\varpi'} = e \frac{p \left\{ v' \left(1 - \frac{p\kappa'}{2} \right) - v \left(1 - \frac{p\kappa}{2} \right) \right\}}{\omega_0 V_0 \alpha T}. \quad \dots \quad (2)$$

For temperatures near 0° C. we may neglect products of p and κ , and we obtain as an approximation

$$\frac{\omega}{\omega'} = 1 + \frac{p(v' - v)}{\omega' V'},$$

or

$$\omega - \omega' = p \frac{v' - v}{V'}. \quad \dots \quad \dots \quad \dots \quad \dots \quad \dots \quad (3)$$

At 0° ,

$$v' - v = 0.087, \quad V' = 209037,$$

and the pressure required to lower the melting-point t° is $\frac{760t}{0.00733}$ millim. by the well-known formula. Substituting in equation (3), we get

$$\omega - \omega' = 0.044t;$$

or

$$\frac{d\omega'}{dt} - \frac{d\omega}{dt} = 0.044 \text{ millim. of mercury}, \quad \dots \quad (4)$$

which is Kirchhoff's result.

If the temperature be much below 0° C., we cannot make these approximations without further examination, as the terms containing κ and κ' in (2) may rise into importance.

It may be noticed that (2) could be used as an equation to determine p , the pressure required to produce a fall of the melting-point to T , if there were any accurate experimental method of measuring ϖ and ϖ' .

Effect of Pressure on the Melting-point.

If we are right in regarding the change from the solid to the liquid state as an exchange phenomenon in which the rate of exchange is indicated by the vapour-tension, we ought to be able to show that the pressure which lowers the melting-point to a certain temperature will so alter the rate at which the two states of the substance give off molecules from their surfaces, that at that temperature there will be an equilibrium of exchange. That is, we ought to be able to show that pressure alters the vapour-tensions of the two states, but alters them by different amounts, so that the equality of vapour-tensions now occurs at the new melting-point.

Now in the ordinary case, where the vapour-tension is measured we have the substance only under the pressure of its own vapour; but in the rise or fall of a liquid in a capillary tube we may have a substance in contact with its own vapour when the substance is at a very different pressure from the vapour in contact with it.

Sir William Thomson has shown (Proc. Roy. Soc. Edinb. 1870, vol. vii. p. 63; Maxwell's 'Heat,' 1877, p. 287) that if a liquid rises in a capillary tube so that its surface is concave upwards, and (we may add) the pressure of the liquid is less than at the plane surface, then the equilibrium vapour-tension is less than at the plane surface. If the liquid falls in the tube, so that the surface is convex and the pressure greater than at the plane surface, then the equilibrium vapour-tension is greater. It has been supposed that this difference of vapour-tensions is due to the curvature of the surface; and Fitzgerald has suggested that we may thus perhaps obtain a connexion between "two apparently unrelated quantities," the evaporation and the surface-tension (Phil. Mag. [5] viii. p. 384). But while a very slight impurity in a liquid can greatly alter

the surface-tension, it has not been shown that it alters the evaporation to the same degree. I think that we must look for the explanation elsewhere than in the curvature of the surface; and I shall endeavour to show that we may account for the effect by the difference of pressures of the liquid at the curved and plane surfaces. The curvature of the surface is then, as it were, an accidental accompaniment of the difference of pressure, and not the cause of the variation in the vapour-tension. We might therefore expect to find the variation taking place also at flat surfaces if the pressure be altered, and with solid as well as with liquid bodies. We cannot directly investigate the vapour-tension of flat surfaces under pressure; but I shall assume that we may here take, instead, the rate at which exchange takes place when the solid and liquid are in contact with each other.

Sir W. Thomson's formula is

$$p = \varpi - \frac{2T\sigma}{r(\rho - \sigma)}, \quad \dots \quad (5)$$

where

p is the vapour-tension in contact with the concave surface,
 ϖ is the vapour-tension in contact with the plane surface,
 T is the surface-tension of the liquid,
 ρ and σ the densities of the liquid and its vapour respectively,
 r the radius of curvature of the curved surface.

If P be the difference between the hydrostatic pressures just beneath the curved surface and just beneath the plane surface, equation (5) may easily be put in the form

$$p = \varpi - P \frac{\sigma}{\rho}, \quad \dots \quad (6)$$

or a pressure P in the liquid increases the vapour-tension by an amount $P \frac{\sigma}{\rho}$.

The following proof of this formula, $p = \varpi - P \frac{\sigma}{\rho}$, is, I believe, applicable to both solids and liquids, and obtains a more general form for the result.

Let a volume v of a body (solid or liquid) be in a perfectly conducting cylinder (fig. 1, Plate XIII.) so arranged that the temperature is always constant. A porous plug, which the substance if liquid does not wet, is in the cylinder; and the holes in the plug are to be so fine that any required pressure can be

applied to the liquid without forcing it beyond the further surface of the plug, the curved surface of the liquid there withstanding the pressure. A piston to which pressure can be applied is in contact with the substance; and beyond the plug is another movable piston to which any pressure can be applied, the arrows in the figure indicating the direction in which the external pressures are applied to the pistons.

Let the volume of the substance in the denser state at the pressure of its normal vapour-tension ω for the given temperature be v . Let V be the volume of the whole as vapour at the pressure ω . Let the equilibrium vapour-tension when the denser state is subjected to a greater pressure $\omega + P$ be p , and let the volume of the whole as vapour at the pressure p be V' . Let the coefficient of cubic compressibility be κ . Now take the body through the following cycle.

Increase the pressure to $\omega + P$ on the left-hand piston, and then let the substance evaporate through the plug to the right hand, pushing out the piston there at pressure p till the whole is evaporated to a volume V' . If p be greater than ω , let the vapour expand from V' , always in equilibrium with the pressure, finally arriving at a volume V and pressure ω . Now cover the porous plug, and, if necessary, commence condensation by introducing a small amount of the substance. Push in the right-hand piston at the pressure ω till the whole is condensed to volume v .

We have now conducted the substance through a cycle each step of which is reversible*. Then

$$\int \frac{dQ}{T} = 0.$$

* It seems difficult to imagine a plug which would satisfy the condition of reversibility for the solid under great pressure in contact with its vapour. Perhaps the following would answer the requirements, if an ordinary porous plug is insufficient. Suppose the solid in a finely-divided state, and contained in a liquid which wets it but is of a very slightly greater specific gravity, and whose vapour-tension is negligible. During evaporation turn the cylinder with the vapour-chamber upwards. The particles of solid will rise up through the pores, and a small fraction of their surface will protrude, but they will otherwise be subjected to a pressure $\omega + p$. For condensation reverse the cylinder. As the solid condenses on the surface it will rise up as fast as it is formed, and so increase the volume of the chamber and force back the piston.

But T is constant; then

$$\int dQ = 0,$$

and the external work is, on the whole, zero. This gives us

$$\left(\omega + \frac{P}{2}\right) P \kappa v + (\omega + P)v(1 - P \kappa v) - pV' - \omega V \log \frac{p}{\omega} + \omega(V - v) = 0. \quad \dots \quad (7)$$

But since, at low temperatures such as we are here considering, Boyle's law is almost exact, we have

$$\omega V = pV'.$$

Then, neglecting terms containing $\omega \kappa$,

$$\omega V \log \frac{p}{\omega} = Pv \left(1 - \frac{P\kappa}{2}\right),$$

or

$$\frac{p}{\omega} = e^{\frac{Pv}{\omega V}} \left(1 - \frac{P\kappa}{2}\right). \quad \dots \quad \dots \quad \dots \quad \dots \quad \dots \quad (8)$$

For ordinary values of P this gives

$$p - \omega = \frac{Pv}{V} = \frac{P\sigma}{\rho}, \quad \dots \quad \dots \quad \dots \quad \dots \quad \dots \quad (9)$$

which agrees with Sir W. Thomson's result in equation (6).

It may be worth while to point out the following result of the reasoning on which the above proof is based.

In a quantity of liquid at a uniform temperature, the number of molecules interchanged across a surface will increase as we descend, owing to the increase of pressure. If near the surface the number be proportional to the vapour-tension at the surface, then at any depth the number will be proportional to the pressure in an atmosphere of vapour at that level which, at the level of the surface, has the pressure of the vapour in equilibrium; that is, the liquid will behave as a non-vaporizing solid through whose interspaces the vapour can move freely.

Assuming, then, that equation (9) holds both for solids and liquids, let us apply it to the case of ice and water in contact with each other at a temperature $-t^\circ$ and at a pressure P , such that $-t^\circ$ is the melting-point.

Let ω be the normal vapour-tension of water at $-t^\circ$,

ω' " " " ice at $-t^\circ$,

p be the altered vapour-tension of water,

p' " " " ice,

ρ the density of water, v its specific volume,

ρ' " " ice, v' " "

σ " " their vapour, V its specific volume.

Taking σ and V as the same for ice and water as an approximation, then equation (9) gives us

$$\left. \begin{aligned} p &= \omega + P \frac{\sigma}{\rho} = \omega + \frac{Pv}{V}, \\ p' &= \omega' + P \frac{\sigma}{\rho'} = \omega' + \frac{Pv'}{V}. \end{aligned} \right\} \quad \dots \quad (10)$$

Subtracting, we have

$$p - p' = \omega - \omega' - P \frac{v - v'}{V}. \quad \dots \quad (11)$$

But by equation (3) we have

$$\omega - \omega' = P \frac{v' - v}{V};$$

then

$$p - p' = 0. \quad \dots \quad (12)$$

Or, under the pressure P at the melting-point, the vapour-tension of ice equals that of water, and there is an equal interchange of molecules taking place. According to this, then, we may thus regard the alteration of melting-point by pressure. The pressure increases the number of molecules given off from the surfaces in contact with each other in both states; but the increase is greater in the case of the less dense state. Now, in the case of ice-water, ice is the less dense state, and below 0° it has the less vapour-tension. Hence a sufficiently great increase of pressure, while increasing both vapour-tensions, can make that of ice overtake that of water, or can lower the melting-point. For paraffin, the liquid is the less dense state. Then, increase of pressure can only render the two vapour-tensions equal above the normal melting-point when the liquid vapour-tension is less than that of the solid.

Suppose now only one of the two states (the ice) to be subjected to increase of pressure. For instance, let the ice be compressed on a porous plate through which the water can circulate freely. Then the pressure increases the rate at which

molecules escape from the ice into the water, but does not affect the rate of escape of the water-molecules into the ice, and a much less pressure will suffice to produce equilibrium of exchange for a given temperature below 0° than when both ice and water are subjected to the pressure.

To calculate the fall in melting-point produced by a pressure P' on the ice alone, we have, instead of (10),

$$\left. \begin{array}{l} p = \varpi, \\ p' = \varpi' + \frac{P'v'}{V}. \end{array} \right\} \quad \dots \dots \dots \quad (13)$$

If we have $p = p'$, we have the melting-point; and in this case, by subtracting, we obtain

$$\varpi - \varpi' = \frac{P'v'}{V}. \quad \dots \dots \dots \quad (14)$$

Now the pressure required to lower the melting-point to the same degree when both ice and water are compressed is given by

$$\varpi - \varpi' = \frac{P \cdot v' - v}{V}; \quad \dots \dots \dots \quad (15)$$

or

$$\begin{aligned} \frac{P'}{P} &= \frac{v' - v}{v} \\ &= 0.087. \quad \dots \dots \dots \quad (16) \end{aligned}$$

Or the fall in melting-point caused by a given pressure on the ice alone is about $11\frac{1}{2}$ times as great as when both ice and water are compressed. That is, 1 atmosphere lowers the melting-point about 0.0843° C., and 11.7 atmospheres lower it 1° C. This result may be obtained in the same way as Prof. J. Thomson's formula, on the supposition that the process is reversible; but as I was led to the result by the above considerations, I have given only this proof.

This seems to have an important bearing on ordinary cases of regelation, when two pieces of ice are brought into contact at one or two points. About that point the ice will be subjected to great pressure; but the melted water is not necessarily subject to the pressure, and accordingly the melting-point may be lowered by $11\frac{1}{2}$ times as much as has been formerly supposed.

Perhaps the following imaginary experiment may serve as a simple illustration of the last two sections. Suppose two

cylinders, one containing ice, the other water at the same temperature, to be connected above by a tube through which the vapour can pass, and let them only be in contact with their own vapour.

At 0° , or rather at $+0.073$, their vapour-tensions being equal, as soon as the pressure reaches 4.6 millim. then the ice and water will remain unaltered in amount as long as no heat is allowed to pass into or out of the cylinders. If the temperature be kept slightly below 0° , then, since the vapour-tension of water is now greater than that of ice, the water will gradually distil over into the ice-vessel and there condense as ice, the average temperature rising. If the temperature be kept constant, however, the whole of the water will in time go over into the ice-vessel. If the temperature be slightly above 0° (supposing it possible still to keep the ice solid), then the ice has the greater vapour-tension and will gradually distil over into the water-vessel, and the average temperature will fall. In time, if the temperature be kept above 0° , the whole of the ice will go over into the water-vessel.

If, now, the ice and water be subjected to pressure by porous pistons which the water does not wet (the pressure in each cylinder being the same), then, if the temperature be 0° , an increase of pressure will cause more evaporation from the ice than from the water; that is, the ice will distil over into the water-cylinder and form water there. To obtain equilibrium again, the temperature must be lowered to such a point that the pressure makes the two vapour-tensions once more equal, when the ice and water will remain unaltered in amount—that is, the melting-point will be reached. If now the ice alone be subjected to pressure, its vapour-tension will be increased while that of the water remains the same. And now the pressure required to produce equilibrium of vapour-tensions at a given temperature below 0° will only be about 2-23rds of that required when both are subjected to the same pressure.

The suppositions which I have made amount to this—that if the space filled with vapour be abolished and the ice and water be brought directly into contact with each other, then the rate of escape of molecules will be the same as before in each case, or bear the same proportion to it.

Isothermals of Ice-water: Critical Points.

If we draw the isothermals for ice and water on a pressure-volume diagram, they are of the general form shown in fig. 2, though the figure is entirely out of proportion.

If we may assume that the compressibility of water is considerably greater than that of ice, the horizontal part of the isothermals representing a mixture of ice and water will increase as the temperature falls below 0° , at least just at first. Then, if we call the line passing through the points where the isothermals turn to or from the horizontal part the ice-water line, this line will at first diverge as the temperature falls. Now, while ice contracts on cooling, its coefficient of expansion between -19° and 0° being given as $.000122$ by Brunner, Despretz has shown that water expands on cooling below 0° even more than it expands for an equal rise above 8° . Hence the isothermals for ice and water approach each other at ordinary pressures as the temperature falls.

Using Brunner's coefficient for ice, and for water Hällström's formula (Jamin, *Cours de Physique*, vol. ii.),

$$\frac{v_0}{v_t} = 1 + .000052939t - .0000065322t^2 + .00000001445t^3,$$

and supposing that water could be cooled without freezing, it will be found that between -120° and -130° ice and water would have the same specific volume. This might lead us to suspect that the divergence of the two branches of the water-ice line would not continue if we could examine the isothermals at very low temperatures and high pressures, and that, as the temperature fell, the two states would at some point begin to approach (that is, the horizontal part of the isothermals would decrease), and that ultimately ice would pass gradually into water without any abrupt change of volume (that is, there would be a critical point). Below this critical point ice and water would probably be identical.

A similar conclusion is arrived at from the latent-heat equation. On the supposition that at the critical point the latent heat vanishes, the temperature given by that equation is $-122^{\circ}.5$, with a pressure of over 16,000 atmospheres (Baynes, 'Thermodynamics,' p. 169).

It is usually assumed that we must stop the isothermal at the base-line of no pressure. But we know that water can be subjected to a negative pressure; as, for instance, when it rises in a capillary tube in a vacuum, or when it adheres to a barometer-tube at a height greater than that of the barometric column. It seems probable that, if perfectly freed from foreign gases, it might even be subjected to a very high negative pressure without the particles being torn asunder. So, too, a mixture of ice and water might probably be subjected to tension. It seems at least worth while to draw the isothermals for ice and water on such a supposition.

Prof. J. Thomson's result for the alteration of the melting-point by pressure would hold for at least a short distance above 0° when we replace pressure by tension. Assuming it to hold for 4° , we should have to put on a tension of $4 \div 00733$ atmosphere = 545 atmospheres. But if the expansion of water under a tension equals its compression under an equal pressure, the expansion is about $\frac{1}{21000}$ per atmosphere*; so that the volume of the water at 4° , under a tension of 545 atmospheres, will be 1.026. The ice, whose volume at 4° under no pressure would be 1.088, probably will not expand nearly so much under tension. The change of volume on melting will therefore probably be not very far from

$$1.088 - 1.026 = .062,$$

against a change at 0° of .087. Then the two branches of the ice-line will converge very considerably for temperatures above 0° and with negative pressures. At this rate of convergence the meeting-point is at about 14° C. At higher temperature the ice would pass gradually into water—that is, we should here have another critical point,—the two critical points being at opposite ends of the closed curve which represents the water-ice line.

On considering the isothermals below 0° , it will be noticed that the water-isothermals, at least as far as that for -20° , can be prolonged downwards past the horizontal line to meet the line of no pressure; for Despretz succeeded in cooling water to -20° in thermometer-tubes without freezing. These pro-

* Might not the truth of this supposition be tested by the propagation of sound through the water above a barometric column at a negative pressure?

longations are represented by $a a'$, $b b'$, $c c'$ (fig. 2). Similarly the ice-isothermals can be at least slightly prolonged upwards past the horizontal line. For, suppose we take a block of ice at 0° and suddenly subject it to great pressure. Since it expands on heating, then sudden compression produces, if any thing, a slight rise in the temperature. At the same time the melting-point is lowered, and the ice begins to melt at the surface, and in time the whole will be lowered to the new melting-point. But just at first, and until it falls to that temperature, we have the ice on the prolongation of the isothermals upwards as at $A A'$ or $B B'$ in fig. 2. In a certain sense, then, we have "hot ice."

Since, then, the water-isothermals may be prolonged downwards and the ice-isothermals upwards, we may probably here adopt Prof. J. Thomson's suggestion as to the true shape of the isothermals in the case of liquid-and-gas mixtures (Brit. Assoc. Report, 1871, p. 30; Maxwell's 'Heat,' p. 125). This is indicated by the dotted line for -2° in the figure. If the isothermals also have this shape above 0° (as indicated by the dotted line for the 4° isothermal), then at first the ice-isothermals will be prolonged upwards to meet the line of no pressure, as, for instance, that of 4° at H. This seems to be the place where we must put Dr. Carnelley's "hot ice," on the diagram, if its temperature be really proved to be above 0° .

But if the critical point for the higher temperature exist, it is evident that, before this temperature is reached, the prolongations of the ice-isothermals will cease to reach up to the line of no pressure, and the limit to the temperature of hot ice in a vacuum is that of the last isothermal which touches the line of no pressure. To obtain ice at still higher temperatures, it would apparently have to be subjected to great tension. If the above calculation for the critical point is at all near the truth, then the highest temperature possible for ice in a vacuum is something below 14° C.

The view here advocated as to the nature of the melting of ice, would show that its fixity is as much a "constant accident" as the fixed boiling-point of water. If we have a piece of ice at any temperature and allow no water to form on its surface, then I see no reason why it should melt if heat be supplied to it by conduction from bodies which, when melted,

it does not wet. I think, then, we ought to expect its temperature to rise, as Dr. Carnelley has apparently found to be the case.

Dr. Lodge has pointed out ('Nature,' Jan. 20, 1881) that, as far as we know, "there is no definite subliming-point for a solid, any more than there is a definite evaporating-point for a liquid." Hence, with such a mode of supplying the heat as above described, the temperature might perhaps be expected to rise to that of the last isothermal which reaches the line of no pressure. When it has reached this point the whole will be in an unstable state, and we might expect a further supply of heat to cause a sudden change into water. If, however, at any point in this process of raising the temperature the vapour-tension is allowed to rise nearly to its maximum, it will exceed that of water, which has a lower maximum; then a layer of water will be formed on the ice, and we shall have melting with a tendency of the temperature towards 0°.

The Sealing-wax Type of Melting.

We have seen that there is some reason to suppose that ice would pass gradually into water at a sufficiently low temperature and with sufficiently high pressure; that is, there would be no abrupt change of volume at a constant temperature, and no definite latent heat. But these are just the characteristics of the melting of substances of the sealing-wax type; and I think it exceedingly probable that we have such substances at temperatures below their critical points, or at least that they are analogous to water-ice below its critical point. If sealing-wax have a critical point, then if we start with some in the solid state at ordinary temperature, and while raising the temperature we increase the pressure so as always to keep it solid till above the critical point, if we reduce the pressure again to a certain point and at the same time a small amount of liquid sealing-wax be introduced, we ought to have a liquefaction of the whole with a finite expansion of volume; that is to say, we should have changed the ordinary sealing-wax type of melting into the ice-water type. It might, perhaps, be possible to test the truth of this supposition experimentally.

XXX. *On the Opacity of Tourmaline Crystals.*By Professor SILVANUS P. THOMPSON, *B.A., D.Sc.***Introduction.*

1. TOURMALINE is distinguished amongst crystals for its remarkable optical properties, particularly its power of polarizing light. It is distinguished moreover by possessing characteristic electric properties. It possesses also a crystallographic interest as furnishing an eminent example of non-superposable hemihedry. There can be little doubt that these remarkable and characteristic qualities are closely related to one another, though as yet very little is known of the nature of this probable connexion. In the present paper an attempt is made to connect the optical and electrical properties of the crystal, by showing that the opacity of the crystal to light polarized in a principal plane of section can be deduced from its electric conductivity.

2. In a paper read before Section A of the British Association at Dublin in 1878, by Dr. O. J. Lodge and myself†, we suggested, as a possible explanation of the phenomena of pyroelectricity in tourmaline, that it might be found to possess unilateral conductivity for electricity—and if for electricity, for heat also. Our experiments on the electric conductivity, however, led to negative results; and in the case of heat-conductivity, the only differences observed of a unilateral kind were such as occurred while the temperature was either rising or falling—not whilst it was constant. Our original suggestion, therefore, was not confirmed, though I obtained instead a result which amounted to the discovery of a new phenomenon, "*the Convection of Heat by Electricity*"—namely, that in a pyroelectric crystal whose temperature is rising, heat flows more easily *with* the electricity than it does *against* the electricity. We were later indebted to Mr. G. F. Fitzgerald, of Trinity College, Dublin, for the suggestion that an analogous phenomenon might occur, namely that there might be a

* Read June 11, 1881.

† Report Brit. Assoc. (Dublin) 1878, p. 495; and Phil. Mag. July 1879.

unilateral electric convection whilst the electromotive force producing the flow was rising or falling*.

3. Whilst we were pursuing these investigations, and before we abandoned our original hypothesis, the suggestion was made to me by Dr. Lodge that if tourmaline possessed unilateral conductivity *along* the axis, this might afford a possible explanation of the greater opacity observed in coloured tourmalines for one set of rays. For if, as on our original hypothesis, the elasticity in one sense along the axis were different from the elasticity in the opposite sense along the axis, vibrations taking place along the axis would be stopped, and the only rays *transmitted* would be those vibrating at right angles to the axis. Unfortunately for this suggestion, the ray whose vibrations are executed in a plane at right angles to the axis is the ordinary ray, which is the one to which the tourmaline is opaque ; whilst the ray which it transmits is the extraordinary ray, which, being polarized in a plane at right angles to the axis, is propagated by vibrations (according to Fresnel and Stokes) executed in a principal plane of section. I could not, therefore, agree with this suggestion as to the cause of opacity in tourmaline crystals, though the suggestion that the opacity was involved in the electrical properties of the crystal appeared an extremely likely one.

4. A theory of the opacity of tourmaline crystals, however, has recently occurred to me whilst considering the general relations of electricity and light ; and I now beg to offer the following explanation, based upon Clerk Maxwell's electromagnetic theory of light.

According to Maxwell's theory, light is an electromagnetic disturbance propagated in the same medium which transmits other electromagnetic actions, the periodic vibrations being propagated in a wave through media possessing "electric elasticity" (*i. e.* dielectric properties), but being frittered down

* We have not yet put this suggestion to experimental proof. In any case the experiment would be difficult, for the reasons alleged in our paper of 1878. Moreover the question would still be complicated by phenomena of heating ; for, as we pointed out in 1878, the phenomena of pyroelectricity must be reversible, and the application of an external electromotive force to a crystal must produce in it a thermal change just the reverse of that which would itself give rise to the electromotive force in question.

into electric currents (and ultimately into heat) when these vibrations pass into conducting media. Good insulators should therefore be transparent, and good conductors opaque, to light. *If it can be shown that tourmaline crystals are better conductors of electricity in one direction than another, it can be deduced as a consequence of Maxwell's theory that they will be better transmitters of light in one direction than in another, and that they will absorb more of those rays of light whose vibrations, consisting of electric displacements, lie in the direction of best electric conductivity.*

The present paper consists of three parts:—

I. A *résumé* of the Optical, Electrical, and Magnetic properties of Tourmaline Crystals.

II. A Mathematical Theory of the Opacity of Crystals which are electrically or magnetically æolotropic.

III. A discussion of the Electric Conductivity of Tourmaline Crystals, and of the Experimental methods of observing it.

I. *Physical Properties of Tourmaline Crystals.*

5. *Optical Properties.*—Tourmaline belongs to the rhombohedral system of crystals, and is therefore optically a uniaxial crystal. Its refractive indices are (approximately)

Ordinary ray 1·6406,

Extraordinary ray 1·6212;

and it is therefore a negative crystal. Tourmalines are of various colours, occasionally colourless, still more rarely pink (rubellite), but frequently bluish-green, green, olive, or brown. The commonest of all kinds is of a brownish tint when cut into very thin slices, but appears exteriorly jet-black. Those of bluish-green or green hue are, according to M. Mascart, the most strongly pyroelectric; and of these the most transparent are more highly pyroelectric than the opaque varieties. *In ordinary light a coloured tourmaline is always found to be much more opaque to rays traversing it in a direction parallel to its axis than to rays traversing it at right angles to its axis, equal thicknesses being considered.* Tourmaline possesses the property of polarizing partially or wholly the light which it transmits. Its action on light is usually explained by saying that a ray of ordinary light incident upon the crystal is divided into two portions, the ordinary and the extraordinary ray,

which in passing through the crystal are unequally absorbed. If a slice of the crystal be taken whose parallel faces are principal planes of section, and which is of a suitable thickness, the ordinary ray will be virtually suppressed, whilst the extraordinary ray is transmitted, though with some loss by absorption. The transmitted ray is polarized in a plane at right angles to the axis of the crystal ; and its vibrations are therefore executed (if we assume with Fresnel and Stokes that the vibrations are at right angles to the plane of polarization) in a direction parallel with that of the axis of the crystal.

We may remark parenthetically, that the almost complete opacity of coloured tourmalines to rays travelling along the axis affords a strong confirmation of the views of Fresnel and Stokes. For let this axis be the x -axis of a system of rectangular coordinates, the transverse vibrations to a ray travelling along x must be in planes parallel to the plane (yz); and they are suppressed by absorption whether parallel to y or z . Now for a ray travelling along y the vibrations are in planes parallel to the plane (xz), and the z -component is suppressed by absorption, the x -component only being transmitted; while for a ray travelling along the axis of z the vibrations are in planes parallel to (yx), and, the y -component being suppressed by absorption, the x -component is alone transmitted.

6. *Electrical Properties.*—With the characteristic pyroelectric properties of tourmaline we have not much concern here; for the tourmalines which are most highly pyroelectric appear to be least opaque.

The electric conductivity of tourmaline is in general very small. The resistance of a centimetre cube of the crystal at ordinary temperatures is certainly many thousand ohms. Gaugain states that the resistance decreases rapidly if the temperature is raised to 400° or 500° , and he has observed* some crystals, on being cooled from this temperature, to retain a high surface-conductivity, which he attributed to their *having become* hygroscopic—an explanation which seems somewhat doubtful, since he states that washing them in ordinary water rendered them non-conducting as before. Becquerel sought to explain the disappearance of pyroelectric phenomena at

temperatures above 150° C. by supposing an increase of conductivity to be assumed at that temperature. In the researches of Dr. Lodge and myself in 1878-9 we found no such great decrease in resistance, which at 300° C. was still enormous. We were not able to compare the conductivity in a direction at right angles to the axis with that along the axis.

The only experiment directed toward the comparison of the electric conductivities in different directions with respect to the axis of the crystal, are those of de Senarmont* and of G. Wiedemann†, both of whom examined the conducting-power of the surfaces of crystals. Their methods and results are examined in detail in the concluding part of this paper. They each found the conductivity of tourmaline to be greater along the axis than across the axis: I shall have occasion for showing that this conclusion is fallacious.

7. *Magnetic Properties.*—Plücker examined the magnetic deportment of tourmaline. The following account of what was observed is given by Tyndall‡:—“A plate of the crystal which had been prepared for the purposes of polarization, 12 millimetres long, 9 wide, and 3 thick, was suspended by a silk fibre between the poles of an electromagnet. On sending a current round the latter, the plate, which was magnetic, set itself as an ordinary magnetic substance would do, with its longest dimension from pole to pole. The optic axis of the crystal, thus suspended, was vertical. On hanging the crystal, however, with its optic axis horizontal, when the magnet was excited, the plate stood no longer as a magnetic substance, but as a diamagnetic; its longest dimension being at right angles to the line joining the poles. The optic axis of the crystal was found to coincide with its length; and the peculiar deportment was considered as a proof that the optic axis was repelled.” From all which the logical inference is, that the coefficient of magnetic induction is less along the axis than in a direction at right angles to it. It is curious that Professor Tyndall states § just the opposite to this—that the maximum magnetic induction is, in coloured crystals at all events, in a direction parallel to the axis. *

* *Ann. de Chim. et de Phys.* t. xxviii. (1850), p. 257.

† *Ibid.* t. xxix. (1850), p. 229; and *Pogg. Ann.* lxxvi. p. 404 and lxxvii. p. 534.

‡ ‘Diamagnetism,’ p. 2.

§ *Op. cit.* p. 71.

8. *Thermal Conductivity.*—De Senarmont examined the thermal conductivity of tourmaline by his well-known method. The conductivity is greater across the axis than along it, the ratio between the major and minor axes of the isothermal curves being (according to my own observations) about 3 : 2; and these being proportional to the square roots of the conductivities, we have

$$3 : 2 = \sqrt{k_2} : \sqrt{k_1},$$

or

$$\text{conductivity along axis : conductivity across axis} = 4 : 9.$$

The unilateral conductivity observed in my experiments during rise or fall of temperature need not be further noticed here.

9. *Radiating-power.*—Kirchhoff* and Stewart† have shown that the law of equality of power to absorb and emit radiations extends to the case of the light absorbed by the tourmaline, which, when heated to redness, emits rays partially polarized in a principal plane of section—that is to say, emits those rays most freely which it most freely absorbs.

II. Mathematical Theory.

10. Let K , μ , and C stand respectively for the dielectric inductive capacity, the magnetic inductive capacity, and the conductivity per unit of volume of a medium. In crystalline media these quantities will, in general, have different values in three principal rectangular directions, being no longer mere scalar quantities, but linear (and vector) operators upon their several functions. If these values be specified for any crystal, then the optical properties of that crystal may be deduced from the general equations of electromagnetic disturbances. These equations are found in Maxwell's 'Electricity and Magnetism,' art. 783 (7), as follows:—

$$\left. \begin{aligned} \mu \left(4\pi C + K \frac{d}{dt} \right) \left(\frac{dF}{dt} + \frac{d\Psi}{dx} \right) + \nabla^2 F + \frac{dJ}{dx} &= 0, \\ \mu \left(4\pi C + K \frac{d}{dt} \right) \left(\frac{dG}{dt} + \frac{d\Psi}{dy} \right) + \nabla^2 G + \frac{dJ}{dy} &= 0, \\ \mu \left(4\pi C + K \frac{d}{dt} \right) \left(\frac{dH}{dt} + \frac{d\Psi}{dz} \right) + \nabla^2 H + \frac{dJ}{dz} &= 0, \end{aligned} \right\} \dots (1)$$

* Pogg. *Ann.* cix. 1860, p. 275.

† Proc. Roy. Soc. x. p. 503.

where

$$\mathbf{J} = \frac{d\mathbf{F}}{dx} + \frac{d\mathbf{G}}{dy} + \frac{d\mathbf{H}}{dz},$$

and where \mathbf{F} , \mathbf{G} , \mathbf{H} are the components of vector-potential at the point (x, y, z) in three rectangular directions parallel to the x , y , and z axes respectively, and where ψ represents the electric (scalar) potential of the possible free electricity. These general equations apply to the case of a medium in which both electric displacements and true conduction currents are simultaneously produced under the action of an impressed electromotive force. They cease to apply in strictness to the case where the medium possesses movements of its own, which we are not here considering. If the medium be homogeneous, whether æolotropic or isotropic, and if its conductivity undergoes no discontinuity in its values at any point (that is to say, has the same continuous values in the same direction), there will be no reason to expect the phenomenon of electric absorption to occur, or free electrification to appear at any internal point of the medium. Hence, in considering steady periodic disturbances such as light-waves, we may neglect terms involving only functions of the free electricity, or its volume-density, such as $\nabla^2\psi$, since these will be independent of t .

11. In order further to simplify the equations, we will narrow down the case to that of a plane wave of unpolarized light propagated along the axis of z , and therefore lying in planes parallel to the plane (xy) , in which planes also the electric and magnetic disturbances are executed. We will further suppose the crystal under consideration to be, as tourmaline is, a uniaxal crystal, and to have its optic axis coincident with the axis of x , so that the x and y components of the disturbances will be respectively parallel and at right angles to the optic axis. We shall also make the *assumption* that in tourmaline the vectors, or linear vector operators, which should stand for \mathbf{K} , μ , and \mathbf{C} , may be represented with sufficient accuracy by assigning to these quantities the values

$$K_1, \quad K_2, \quad K_3,$$

$$\mu_1, \quad \mu_2, \quad \mu_3,$$

$$C_1, \quad C_2, \quad C_3,$$

for their respective values as measured in the directions of x , y , and z .

We may then write, neglecting functions of J , which in the case of periodic disturbances can at most be a linear function of the time,

$$\left. \begin{aligned} 4\pi\mu_1 C_1 \frac{dF}{dt} + \mu_1 K_1 \frac{d^2F}{dt^2} + \nabla^2 F &= 0, \\ 4\pi\mu_2 C_2 \frac{dG}{dt} + \mu_2 K_2 \frac{d^2G}{dt^2} + \nabla^2 G &= 0, \\ 4\pi\mu_3 C_3 \frac{dH}{dt} + \mu_3 K_3 \frac{d^2H}{dt^2} + \nabla^2 H &= 0. \end{aligned} \right\} \quad \dots \quad (2)$$

Since, however, we are going to deal only with disturbances in the plane (xy) propagated along the axis of z , we may simplify the above to the following form:—

$$\left. \begin{aligned} 4\pi\mu_1 C_1 \frac{dF}{dt} + \mu_1 K_1 \frac{d^2F}{dt^2} - \frac{d^2F}{dz^2} &= 0, \\ 4\pi\mu_2 C_2 \frac{dG}{dt} + \mu_2 K_2 \frac{d^2G}{dt^2} - \frac{d^2G}{dz^2} &= 0, \\ 4\pi\mu_3 C_3 \frac{dH}{dt} + \mu_3 K_3 \frac{d^2H}{dt^2} &= 0. \end{aligned} \right\} \quad \dots \quad (3)$$

Or, in words, the rate of change in the transverse components of electromotive force as the wave advances along the axis of z is expressed as the sum of two time-functions of the corresponding components of vector-potential—one of these functions varying with the electric conductivity of the medium, the other with its dielectric capacity, and both being proportional also to the magnetic inductive capacity.

12. We have now four possible cases to consider:—

- (i) where K is very great and C relatively negligible, as in non-conducting media;
- (ii) where C is great and K relatively small, as in true conductors of electricity;
- (iii) where C and K are magnitudes of comparable order, but having different values in different directions.
- (iv) where μ has values differing appreciably in different directions.

Although from Plücker's experiments (see § 7 *ante*) it ap-

pears that μ_1 and μ_2 cannot be regarded as equal, yet their values are both approximately so nearly equal to unity that we are not in a position to discuss case (iv).

13. Case i. (*Non-conducting Substance*), $C=0$.

Equations (2) become

$$\left. \begin{array}{l} \mu_1 K_1 \frac{d^2 F}{dt^2} - \frac{d^2 F}{dz^2} = 0, \\ \mu_2 K_2 \frac{d^2 G}{dt^2} - \frac{d^2 G}{dz^2} = 0, \\ \mu_3 K_3 \frac{d^2 H}{dt^2} = 0; \end{array} \right\} \quad \dots \dots \quad (4)$$

whence

$$\left. \begin{array}{l} \frac{1}{\mu_1 K_1} \cdot \frac{d^2 F}{dz^2} = \frac{d^2 F}{dt^2}, \\ \frac{1}{\mu_2 K_2} \cdot \frac{d^2 G}{dz^2} = \frac{d^2 G}{dt^2}; \end{array} \right\} \quad \dots \dots \quad (5)$$

whence it follows that a plane wave in the plane (xy), whose electric displacements are parallel to x , is propagated with velocity V_1 ; whilst displacements parallel to y are propagated in a wave whose velocity is V_2 with the following values:—

$$\left. \begin{array}{l} V_1 = \frac{1}{\sqrt{\mu_1 K_1}}, \\ V_2 = \frac{1}{\sqrt{\mu_2 K_2}}. \end{array} \right\} \quad \dots \dots \quad (6)$$

This agrees with the known fact that a wave of light is split on passing through the crystal into two portions whose vibrations are respectively at right angles to one another, and which move with different velocities.

Now assuming, as above, $\mu_1 = \mu_2 = 1$, we have

$$\frac{V_1}{V_2} = \frac{\sqrt{K_2}}{\sqrt{K_1}} = \frac{\rho_2}{\rho_1}; \quad \dots \dots \quad (7)$$

where ρ_2, ρ_1 are respectively the indices of refraction for ordinary and extraordinary rays.

Billet gives (*Optique Physique*, p. 619) the following values for the refractive indices of different specimens of tourmaline:—

Colour of ray.	Ray used.	Ordinary index (ρ_2).	Extraordinary index (ρ_1).	$\frac{\rho_2}{\rho_1}$
Colourless ...	Ray D	1.6366	1.6193	1.0107
Green.....	Ray D	1.6408	1.6203	1.01265
Green-blue...	Red ray	1.6415	1.623	1.01311
Blue	Red ray	1.6435	1.6222	1.01313
Mean		1.6406	1.6212	1.0120

14. From these we deduce for the two velocities of light in tourmaline:—

Ordinary Ray.

$$V_2 = \frac{3 \times 10^{10} \text{ cm. per sec.}}{1.6406} = 1.8286 \times 10^{10} \text{ cm. per sec.};$$

and

Extraordinary Ray.

$$V_1 = \frac{3 \times 10^{10} \text{ cm. per sec.}}{1.6212} = 1.8504 \times 10^{10} \text{ cm. per sec.}$$

15. The two corresponding dielectric capacities should therefore be

$$K_1 = \rho_1^2 = 2.6283 \text{ in direction parallel to crystallographic axis,}$$

$$K_2 = \rho_2^2 = 2.6792 \text{ in direction perpendicular to crystallographic axis.}$$

I am not aware of any experimental determination of these quantities having been made.

16. Case ii. (*Conducting Medium*).

Here $\psi = 0$, and we may neglect the terms containing K ; for, though $K > 1$, C is by hypothesis much greater, and the terms containing C are therefore those by which the mode of propagation of electric displacements in the medium are represented. So the equations (3) become

$$\left. \begin{aligned} 4\pi\mu_1 C_1 \frac{dF}{dt} - \frac{d^2F}{dz^2} &= 0, \\ 4\pi\mu_2 C_2 \frac{dG}{dt} - \frac{d^2G}{dz^2} &= 0, \\ 4\pi\mu_2 C_3 \frac{dH}{dt} &= 0, \end{aligned} \right\} \dots \dots \dots \quad (8)$$

which are not the equations of any kind of wave-propagation, but represent as going on in the plane (xy) the diffusion of electricity by conduction through the medium at logarithmic rates whose values along the x and y axes are proportional to $-\mu_1 C_1$ and $-\mu_2 C_2$ respectively; that is to say, the disturbances will be propagated just in the same fashion as heat would be in a medium whose thermal conductivities and specific heats had different values in different directions. The spread of electricity in this case resembles therefore that of the spread of heat in a thin film when the heat starts at a point and diffuses around. In its generality the case is therefore comparable, as Maxwell points out, to that of which Fourier gave the complete solution (art. 384, 'Analytical Theory of Heat,' p. 382, Freeman's translation), for the diffusion of heat in all directions, where the temperature is represented by the triple integral

$$\iiint \frac{d\alpha d\beta d\gamma}{2^3 \sqrt{\pi^3 k^3 t^3}} \cdot e^{-\frac{(\alpha-x)^2 + (\beta-y)^2 + (\gamma-z)^2}{4kt}} \cdot f(\alpha, \beta, \gamma),$$

in which, if we write $r = \sqrt{(\alpha-x)^2 + (\beta-y)^2 + (\gamma-z)^2}$, the exponential $e^{-r^2/4kt}$ will represent the value contributed to the mean temperature at the point (x, y, z) whose distance from the origin is r . In the present case of diffusion of electric currents in the plane (xy), a similarly constructed double integral may be employed to represent the distribution of vector-potential, the quantity symbolized by k (the coefficient of the intrinsic rate of diffusion) being replaced by $\frac{1}{4\pi\mu_1 C_1}$ and $\frac{1}{4\pi\mu_2 C_2}$ in the exponential.

Now, where the whole energy of the electromagnetic disturbances is thus converted in the conducting medium into the energy of currents diffused through its substance, no wave will be propagated; or, as Maxwell has shown, the substance will be opaque to light, the energy of the diffused currents being frittered away into heat within the conducting medium by reason of the resistance encountered.

17. Case iii. (*Medium possessing both Dielectric Inductive Capacity and Conductivity, the values of K and C being of comparable order of magnitude, but having different values in different directions*).

We have from § 11) the equations

$$\left. \begin{aligned} 4\pi\mu_1 C_1 \frac{dF}{dt} + \mu_1 K_1 \frac{d^2F}{dt^2} &= \frac{d^2F}{dz^2}, \\ 4\pi\mu_2 C_2 \frac{dG}{dt} + \mu_2 K_2 \frac{d^2G}{dt^2} &= \frac{d^2G}{dz^2} \\ 4\pi\mu_3 C_3 \frac{dH}{dt} + \mu_3 K_3 \frac{d^2H}{dt^2} &= 0. \end{aligned} \right\} \dots \quad (3 \text{ bis})$$

F and G being the components of vector-potential in the x and y directions respectively, the problem is to determine at what rate these will be diminished by absorption in passing through a given thickness z of the medium when the conductivities per unit volume in those directions are respectively C_1 and C_2 . The proportion borne by the transmitted electromagnetic displacements of the luminous wave to the original displacements, after traversing a thickness z of the crystal, will (neglecting the small proportion lost by surface reflexion) be expressed by an exponential of the form

$$e^{-pz}.$$

where p is the coefficient of absorption. This coefficient Maxwell has calculated (art. 798) for the case of isotropic conducting media, from the general fundamental equation, on the assumption that the disturbance may be expressed as a circular function of the form $\cos(nt - qz)$. Following the lines thus laid down, we will write:—

$$\left. \begin{aligned} F &= e^{-p_1 z} \cos \frac{2\pi}{\lambda_1} (V_1 t - z), \\ G &= e^{-p_2 z} \cos \frac{2\pi}{\lambda_2} (V_2 t - z), \end{aligned} \right\} \dots \dots \dots \quad (9)$$

where p_1 and p_2 are coefficients of absorption in the x and y directions respectively, and where V_1 and V_2 are the two velocities of propagation (see § 13), and where λ_1 and λ_2 are the corresponding lengths of waves. Now, putting

$$\left. \begin{aligned} q_1 &= \frac{2\pi}{\lambda_1}, \\ q_2 &= \frac{2\pi}{\lambda_2}, \end{aligned} \right\} \dots \dots \dots \dots \dots \quad (10)$$

and

$$\left. \begin{aligned} n_1 &= \frac{2\pi V_1}{\lambda_1}, \\ n_2 &= \frac{2\pi V_2}{\lambda_2}, \end{aligned} \right\} \quad \dots \quad \dots \quad \dots \quad \dots \quad \dots \quad (11)$$

whence

$$\left. \begin{aligned} V_1 &= \frac{n_1}{q_1}, \\ V_2 &= \frac{n_2}{q_2}, \end{aligned} \right\} \quad \dots \quad \dots \quad \dots \quad \dots \quad \dots \quad (12)$$

we may write equations (9) in the form

$$\left. \begin{aligned} F &= e^{-p_1 z} \cos(n_1 t - q_1 z), \\ G &= e^{-p_2 z} \cos(n_2 t - q_2 z); \end{aligned} \right\} \quad \dots \quad \dots \quad \dots \quad \dots \quad \dots \quad (13)$$

in which case the expressions (9) will satisfy the differential equations (3), provided that

$$\left. \begin{aligned} 2p_1 q_1 &= 4\pi\mu_1 C_1 n_1, \\ 2p_2 q_2 &= 4\pi\mu_2 C_2 n_2. \end{aligned} \right\} \quad \dots \quad \dots \quad \dots \quad \dots \quad \dots \quad (14)$$

It follows at once that the coefficients of absorption are

$$\left. \begin{aligned} p_1 &= 2\pi\mu_1 C_1 V_1, \\ p_2 &= 2\pi\mu_2 C_2 V_2. \end{aligned} \right\} \quad \dots \quad \dots \quad \dots \quad \dots \quad \dots \quad (15)$$

Here C_1 and C_2 are volume-coefficients of conductivity, and would require to be replaced by their values l/bzR_1 and l/lzR_2 if a given rectangular plate of the substance, of length l (parallel to x), breadth b , and thickness z , were measured, and found to have resistances of R_1 and R_2 respectively in directions parallel to x and y . In the actual case of the tourmaline, however, in which no measurements of resistance have yet been made, the volume-conductivities C_1 and C_2 may stand for conductivities in general. And the proportions of the incident (unpolarized) light transmitted by the crystal will be as follow:—

Ordinary ray (polarized in a plane (xz) of principal section; electric displacements parallel to y , magnetic displacements parallel to x),

$$\text{proportion transmitted} = e^{-2p_1 z} = e^{-4\pi\mu_1 C_1 V_1 z}.$$

Extraordinary ray (polarized in a plane (yz) perpendicular

to the optic axis ; electric displacements parallel to x , magnetic displacements parallel to y),

$$\text{proportion transmitted} = e^{-2p_1 z} = e^{-4\pi\mu_1 C_1 V_1 z}.$$

If O represent the intensity of the transmitted ordinary ray and E that of the transmitted extraordinary ray, we have

$$\frac{O}{E} = \frac{e^{-\mu_2 C_2 V_2}}{e^{-\mu_1 C_1 V_1}}, \quad \dots \dots \dots \quad (16)$$

whence

$$\frac{\log O}{\log E} = \frac{\mu_1 C_1 V_1}{\mu_2 C_2 V_2}. \quad \dots \dots \dots \quad (17)$$

Now the ratio of V_1 to V_2 is known (§ 14), the velocity of the ordinary ray V_2 being to that of the extraordinary ray V_1 in the ratio 1.8286 to 1.8504. If, therefore, the magnetic permeability were equal both along and across the axis of the crystal, and if the electric conductivity were equal in both directions, the relative intensities of the two transmitted rays would depend only on the relative velocities ; and as V_1 is greater than V_2 , the ordinary ray should be more freely transmitted than the extraordinary ray, which is contrary to observation. The conductivities therefore cannot be equal in fact.

Billet's values (§ 13) for the refractive indices of tourmalines of different colours show slight differences, the colourless tourmaline being apparently a little less refringent than the coloured crystals. As in this case there was no opacity, the presumption is that both C_1 and C_2 were = 0. If the fact were confirmed by observation generally that the index of refraction is less in crystals exhibiting no coloration, we should be obliged to consider K as no longer independent of C in the general equations of electromagnetic disturbances ; and V , which is taken $= (\mu K)^{-\frac{1}{2}}$, would require a more complicated formula for its complete expression.

18. If, again, either the coefficient of magnetic induction or the conductivity be greater along the axis of the crystal than across it, then the ordinary ray should be transmitted more freely than the extraordinary. This is not the fact with *any* tourmalines ; it is always the ordinary ray which suffers the greater absorption.

As I understand Plücker's experiment on the diamagnetism of tourmaline (§ 7 *antè*), the crystal examined by that dis-

tinguished physicist possessed a greater magnetic inductive capacity in a direction *across* the crystallographic axis than in the direction of that axis. If this observation were also confirmed for tourmalines in general, we should have some reason to expect that crystals possessing electric conductivity, even if the conductivity were equal in all directions, should show greater absorption for the ordinary ray; for if $C_1 = C_2$, equation (15) becomes

$$\frac{\log O}{\log E} = \frac{\mu_1 V_1}{\mu_2 V_2} \cdot \cdot \cdot \cdot \cdot \cdot \quad (18)$$

But μ_1 and μ_2 are so nearly equal to 1 in all cases hitherto observed, that the ratio of μ_1 to μ_2 is far less than that of V_2 to V_1 ; and therefore the greater opacity to the ray cannot be accounted for on the supposition that it depends on differences in the magnetic inductive capacity.

19. There appears, then, to be no other explanation of the observed opacity of coloured tourmaline crystals for rays polarized in a principal plane of section than that afforded by the hypothesis that the electric conductivity is greater in directions at right angles to the crystallographic axis than along it. One fact of some importance bearing upon this point is an observation which I have made, that the more opaque the crystal in general the greater is the inequality of absorption of the ordinary and extraordinary rays. The green tourmaline "C" in my possession, which is about 2 millim. thick, transmits both rays partially; but cuts off about three fourths of their intensity from ordinary rays, and above half their intensity from extraordinary rays. It is about 30 millim. in length, and appears to be perfectly opaque in that direction. On the other hand, a slice of a jet-black tourmaline, the property of University College, Bristol, not more than 0.2 millim. in thickness, is perfectly opaque to the ordinary ray; but it transmits extraordinary rays at about half of their intensity. Not only must the conductivity as a whole be higher in the latter case, but the ratio of the two conductivities must be higher in the latter than in the former; and the transverse conductivity must be greater than the longitudinal. The latter crystal was too small to permit of verification of these inferences by direct experiment.

III. *Electric Conductivity of Tourmaline Crystals.*

20. The conducting-power of the tourmaline for electricity is very feeble; and the only determinations that have been made of its relative conducting-powers in longitudinal and transverse directions are determinations, not of the flux of electricity through a slab or film of the substance, but of the superficial conductivity along a face of the crystal. This superficial conductivity has been examined by two slightly different processes, by de Senarmont* and by Wiedemann†.

21. De Senarmont's process consisted in covering the face of the crystal with tinfoil through which a truly circular hole had been punched. The foil was fixed over the crystal with gum or varnish, and was connected to earth. At the centre of the exposed circle a metallic point was fixed in an insulating support; and by this means the electricity stored in a Leyden jar could be made to flow across the circular space from the centre to the circumference. With powerful discharges from a Leyden battery sparks leapt along the crystal face, leaving a permanent trace along the path of least resistance. Later experiments were conducted in a partial vacuum, when the discharge took place quietly in luminous streaks of a pale-violet colour along that diameter of the circle which corresponded to best conductivity along the surface.

The crystals operated upon by de Senarmont were three black tourmalines from Greenland, in the collection of the École des Mines, having plane mirror-like faces tangential to the lateral edges of the primitive rhombohedron and slightly striated ("stries à peine sensibles"). With circles of three different sizes, de Senarmont found the maximum conductivity to lie in a direction parallel to the axis.

22. Wiedemann's process consisted in producing an electric "figure" upon the surface of the crystal, which was dusted over with powdered sulphur, or lycopodium, or red-lead, and then exposed to the discharge of electricity from a finely-pointed conductor close to the surface. The powder was repelled from the point, and heaped itself all round a clear space. In the case of equal conductivities this space was a

* *Ann. de Chim. et de Phys.* 3^e série, t. xxviii. p. 257.

† *Ibid.* t. xxix. p. 229; and *Pogg. Ann.* lxxvi. p. 404, and lxxvii. p. 534.

circle; in the case of unequal conductivities an ellipse whose major axis indicated the greater conductivity. Wiedemann only examined one specimen of tourmaline. He states that "on the faintly striated prismatic faces of the crystal employed, the larger diameter of the electric figure shows itself parallel to the principal axis." He adds, that in the case of other optically negative crystals (including calc-spar) the electric conductivity is greater along than across the axis. (He notes *rutile* as an exception.) But he appends, as a general conclusion, that the electricity is propagated in crystals more rapidly in that direction in which the propagation of light is the more rapid; which dictum exactly contradicts his own observations on calc-spar and on tourmaline, in which the velocity of the extraordinary ray is greater than that of the ordinary ray.

23. Now, if we are to accept these observations on the superficial conduction of natural tourmaline crystals with striated faces as establishing the electric conductivity of tourmaline to be a maximum along the axis and a minimum across it, we shall have to admit that, of all substances examined, tourmaline forms the solitary exception to the rule that the thermal conductivity and the electric conductivity of substances go together *pari passu**. There is no uncertainty as to the thermal conductivity of tourmaline, its transverse conductivity being about twice as great (see § 8 *antè*) as its longitudinal conductivity.

24. Both observers stated that the faces of the crystals were *striated*. De Senarmont appeared, indeed, to think that natural striation interfered less with the regularity of conduction than the invisible striations of an artificially polished face†. I am inclined to think from my own observations that, on the con-

* See Tait, Trans. Roy. Soc. Edinb. 1878, where, however, it appears that German silver, while superior in thermal conductivity to lead, is inferior to that metal in electric conductivity, being the single known exception to Forbes's rule.

† "Quant aux stries et aux aspérités naturelles, elles ne semblent pas avoir une influence appréciable tant qu'elles ne sont pas extrêmement prononcées. J'ai donc autant que possible, pour être sûr des résultats, cherché à opérer sur des faces naturelles ou obtenus par le clivage." *Loc. cit.* p. 264.

trary, the natural striations of the crystal greatly affect the conditions of surface-conductivity. I have repeated both methods of experiment upon tourmalines with natural and with artificially-cut faces. In the former case, the crystals tried were black striated crystals from Norway. They showed, as did the crystals used by de Senarmont and by Wiedemann, a longitudinal conductivity higher than the transverse conductivity. I cleaned one of these crystals, first with hydrochloric acid, and then by rubbing its surface in boiling paraffin. When cold it was carefully cleaned with a piece of dry leather, and the experiments were repeated. De Senarmont's method now showed a transverse conductivity, while Wiedemann's method gave an indecisive result.

25. I therefore tried another method, as follows :—A small circular drop of pure olive-oil was deposited upon the middle of the face of the crystal. A pointed needle held in a non-conducting support was fixed centrally above it, and then the knob of a charged Leyden jar was cautiously approached. The oil-drop spread out somewhat irregularly across the surface; and at its centre appeared a clear space from which the oil was repelled. This clear space was distinctly elliptical, the major axis of the ellipse being transversely placed across the axis of the crystal. The natural striations, however, rendered the form irregular. When the artificially-cut green tourmaline ("C") mentioned in § 19 was employed, the elliptical space in the centre of the oil-drop was much more satisfactorily observed. The ratio of the minor axis to the major axis was not greater than 1 : 2.

It is greatly to be wished that some more exact and more reliable measurements were made of the conductivity of tourmaline in the two principal directions along the axis and across it. Although these more reliable measurements are still wanting, I am disposed to think that my own experiments, backed by the analogy of the thermal conductivity (about which there is no uncertainty whatever), are sufficient to establish qualitatively that the electric conductivity of tourmaline is greater in directions at right angles to the axis than in the direction of the axis. If this be established, the theory of opacity advanced above is proved.

26. There is, indeed, one other possible way of accounting

for opacity in a homogeneous medium besides the supposition made above, that the medium should possess electric conductivity of one kind or another. This alternative way of explaining opacity supposes that a luminous oscillation may pass into a calorific one of the same period. The vibrations of light being regarded as due to rapid alternations in vector-potential, the particles of a medium transmitting the luminous waves must be subjected to alternately directed electromotive forces. Now let there be in the medium a particle of matter whose natural period of oscillation coincides with that of the wave. This particle will be subjected to alternate inductions which will displace electricity in it from side to side, the (electro-) kinetic energy of the movement changing to (electro-) potential energy and back again to the kinetic form at each half swing, as in the alternations of the swinging pendulum. There is no difficulty in understanding that, under this action, the mass of the molecule will be set swinging. It is easy to set a pendulum, consisting of a metal ball hanging by a silk thread, into vibration by subjecting it to periodic electric inductions whose period agrees with that of the pendulum. Thus the (electro-)kinetic energy of the wave passes over into the (pondero-)kinetic energy of the oscillating particle ; which is merely another way of saying that the light is absorbed and changed into heat. This action, however, can only account for absorption on the assumption that the medium is homogeneous, and its individual particles themselves conductors. But if the medium be homogeneous, all its particles will have similar periods of oscillation. This hypothesis may therefore account for selective absorption in media, though it is not adequate to explain general absorption. If the medium be not homogeneous, a *third* explanation of opacity is possible. In a *non-homogeneous* medium there is no need to suppose conductivity to exist; for heterogeneity of structure affords in itself a possible explanation of electric absorption ; and if electric absorption were possible in the medium, the variations of vector-potential would no longer be propagated without loss through the medium, any more than they can be through a medium possessing true conductivity. And if, by reason of structural differences in different directions (differences of fibre, lamination, cleavage, and the like), the degree of electric

absorption were different in different directions, then such a medium might possess also a directed opacity. But such an opacity would be limited by the limitations of electric absorption in general, and would probably be quite distinguishable from the opacity arising from conduction in the medium. Were electric absorption possible, we could no longer omit functions of ψ , the (scalar) electric potential, from the general equations of electromagnetic disturbances assumed in § 10 and in the succeeding development of the theory.

27. The main argument for rejecting the supposition that electric absorption can account for opacity is derived from the consideration that such a medium, after having absorbed charges of opposed signs at different surfaces of its heterogeneous structure, would, when left to itself, slowly recover; and would give rise to phenomena of recovery, variations of vector-potential taking place in the medium in inverse order after the lapse of time. Whether this action may furnish a possible explanation of the phenomena of phosphorescence (*Nachleuchtung*), is a suggestion which may be worth future consideration. Whether the optical properties of tourmaline are to be regarded as dependent on its conductivity or on any other physical property, the physical property on which they depend is certainly a directed property; and its action is such that the crystal possesses what Maxwell called "electric elasticity" in a longitudinal and not in a transverse direction. Now, as those bodies which possess "electric elasticity" are observed for the most part to phosphoresce under the impact of electrified molecules in the negative glow produced in very attenuated vacua such as Crookes has obtained, whilst those bodies which do not possess this property do not phosphoresce under the same circumstances, I venture to predict that, if a slice of tourmaline cut parallel to the axis be found to phosphoresce at all when exposed to the negative discharge, its phosphorescence will be found to consist chiefly of rays polarized in a plane at right angles to the axis—or, in other words, that it will emit the same kind of rays as it transmits. Whether this prediction be verified or not, however, does not affect the validity of the theory of opacity of tourmaline crystals here advanced.

June 4, 1881.

XXXI. Electrostatic Investigations, especially relating to the Division of Induction in the Differential Inductometer and in the Electrophorus. By Dr. JAMES MOSER.

The Differential Inductometer.

IN the year 1838 Faraday completed his experiments on Specific Inductive Capacity, thus virtually repeating the investigations of Cavendish, published some sixty years before. At the end of his communication Faraday gave a description of an apparatus, the "Differential Inductometer," for the determination of such inductive capacity.

This serves as the point of departure for the following investigation; but I shall first, and chiefly, consider the electrostatic induction in a single medium, namely the atmospheric air; so that the question of "specific induction," a question involving more than one medium, will be considered later.

Faraday, in his 'Experimental Researches' (§ 1307), describes the above-mentioned apparatus as follows:—"Three circular brass plates about five inches in diameter were mounted side by side upon insulating pillars. The middle one 'A' was a fixture; but the outer plates 'B' and 'C' were moveable on slides, so that all three could be brought with their sides almost into contact or separated to any required distance. Two gold leaves were suspended in a glass jar from insulated wires. One of the outer plates, 'B,' was connected with one of the gold leaves, and the other outer plate with the other gold leaf. The outer plates B and C were adjusted at the distance of an inch and a quarter from the middle plate 'A'; and the gold leaves were fixed at two inches apart. 'A' was then slightly charged with electricity, and the plates 'B' and 'C' with their gold leaves thrown out of insulation *at the same time*, and then left insulated. In this state of things 'A' was charged positive inductively, and B and C negative inductively; the same dielectric (air) being in the two intervals, and the gold leaves hanging, of course, parallel to each other in a relatively unelectrified state."

Faraday affected the relation of the induction from A to B to the induction from A to C by the introduction of various

dielectrics. I vary such relationship by altering the distance between B and C from A.

Faraday brought a plate of shellac between the middle plate A and one of the outer plates, B. Thereupon the gold leaves approached one another. The leaves again separated when the shellac was taken away. According to Faraday, the attraction is due to the increased induction where the shellac is, causing A to induce more negative electricity on B. The side therefore of B (and its gold leaf) furthest from A becomes positive. On the other outer plate C there is less negative electricity induced; and the outer side of this plate, together with the gold leaf connected with it, becomes therefore negative. Hence the gold leaves attract one another.

In my experiments I employed three zinc plates of 30 millim. diameter, and made use of, on some occasions, a glass jar (described above) having the two insulated gold leaves. Sometimes, however, I used two electroscopes, one of which was connected with the one and the other with the other outer plate. Under these conditions the kind of electricity which was to be found on the plate was easily determined.

I should like to be allowed to mention a few details which I think are of interest in electrostatic investigations. The insulation of the wire to which the gold leaves were hung was effected simply by passing the wire through a tube of ebonite having an external diameter of 5 millim. and 1 millim. opening.

In order to avoid binding-screws, the numerous edges of which are troublesome in electrostatic experiments, connexions were made by boring conical holes of diameters from $\frac{3}{4}$ tapering to $\frac{1}{2}$ millim., and simply thrusting the wires into the holes. No electricity must be allowed to settle on the glass of the electroscope. I therefore employed an ordinary cylindrical glass 15 centim. high and 10 centim. wide, but led round the glass, inside and outside, a continuous strip of tinfoil 3 centim. wide, parallel to the gold leaves and connected with the earth. By this means the electricity of the leaves induced electricity of the opposite kind in the tinfoil; and if this charge became too strong, the leaves touched the tinfoil and discharged themselves. The glass was provided with a wooden cover, through which passed the little ebonite tube

enclosing the wire. Each zinc plate was fastened to a horizontal Z made of shellac. The three Z's overlapped one another, so that the plates could be brought to within 1·5 centim. of one another.

Division of Induction.

The three plates of the differential inductometer are to be looked on as two condensers which have one plate, namely the collecting-plate, in common. From this plate, as "inductive," the induction begins, and ends on the two side plates as "inducteous." In this arrangement the division of the induction corresponds to the division of the current in flowing electricity.

But before I proceed further with the physical considerations, which would appear hypothetical, I shall deduce the mathematical from the theory of potential.

If we look upon the differential inductometer as formed of two condensers, the total electricity, E , on the middle plate consists of two parts, e_1 and e_2 ;

$$E = e_1 + e_2 \quad \dots \quad \dots \quad \dots \quad \dots \quad \dots \quad (1)$$

so that e_1 is the charge on one plate, and e_2 on the other. The value of each of these is found in the following manner. According to Green*, if e be the charge on a plane condenser whose plates have the surface S and the distance r , and if the potential-function on the one is V , while the other is connected with the earth, then

$$e = \frac{VS}{4\pi r}.$$

In the derivation of this equation it is assumed that r is small in comparison with the diameter of the plates, an assumption to which I will return afterwards.

We will apply this formula to the two condensers of the differential inductometer. All three plates have the same surface S . The middle one has the potential V ; the two outer ones are connected with the earth. Let the distance of the plate of which the charge is e_1 be r_1 , that of the one with charge e_2 be r_2 . Then

$$e_1 = \frac{VS}{4\pi r_1},$$

* Green, *Essay*, § 8; Thomson, *Papers*, § 53.

and

$$e_2 = \frac{VS}{4\pi r_2},$$

and, accordingly,

$$e_1 : e_2 = \frac{1}{r_1} : \frac{1}{r_2} \dots \dots \dots \dots \dots \quad (2)$$

In other words, the charges are inversely proportional to the distances. The values of the two charges are derived immediately from equations (1) and (2). According to these,

$$e_1 = \frac{r_2}{r_1 + r_2} E \dots \dots \dots \dots \dots \quad (3)$$

$$e_2 = \frac{r_1}{r_1 + r_2} E. \dots \dots \dots \dots \dots \quad (4)$$

These formulæ, which are the same as those for the divided current, are only absolutely true for plates of an infinite size. They signify that the inductive capacities of the two condensers which form the differential inductometer are inversely proportional to the distances of the plates. If we use the term "inductive resistance" to denote the reciprocal of the inductive capacity in the same way as conductivity is the reciprocal of resistance, then, in the case of infinite plates, the inductive resistance is directly proportional to the distance. In such a case of infinite plates the equipotential surfaces are planes parallel to the plates, and the tubes of force are cylinders at right angles to them.

This electrostatic condition is converted into the corresponding electrodynamic one if a conducting liquid is placed between the plates. If, then, the plates are kept at the same electrical level by (say) a battery, then a divided current would pass from the middle plate through the liquid to the two side plates; so that the middle plate would be an anode, and the side plates cathodes; and the division would then take place in such a manner that the strength of the two branch currents would be inversely proportional to the respective resistances; or, since the latter are proportional to the distances, the currents would be inversely as the distances*. Just as we incur an error when we pass from the law that the divisions of the current are inversely proportional to the

* See Currents between Plates, Wied. *Galv.* § 116.

distances in the case of infinitely large plates, to the case where the plates are simply very large, so we incur a similar error in electrostatics when we apply the formula for infinitely great plates to the inductometer.

The case in electrodynamics which corresponds to the inductometer (which we regard as an apparatus for the division of induction) would be the branching of the current between three round plates. If we imagine the differential inductometer to be plunged into a conducting liquid and the potentials to be constantly maintained, then a current would pass from the middle plate as anode to the external plates as kathodes. Further, if in both cases we construct the system of the surfaces of equal potential, then, if their equation be

$$V = \text{constant},$$

V in both cases (that is, by the branching of the current and in the inductometer) would satisfy the same differential equation, and the same limiting conditions must be fulfilled. We should get in both cases the same system of surfaces of electrical level; and the perpendiculars to these surfaces would have in the one case to be regarded as lines of force, and in the other as lines of flow. The greater the plates are, the more nearly flat are the surfaces of electric level, and the straighter are the lines of force.

Induction and conduction act, therefore, in the same lines. If, in order to fix our ideas, we take spermaceti as an intervening plate, as Faraday did, we find, with him, "spermaceti to be a dielectric, through which induction can take place" (Exp. Res. § 1322). But "spermaceti is also a conductor, though in so low a degree that we can trace the process of conduction, as it were, step by step through the mass. . . . Here induction appears to be a necessary preliminary to conduction" (§ 1323). And although Faraday could not, as we have done, formalize the division of the induction in the same way as the branching of the current is formalized, still he had this agreement in his mind. That he could not formulate this idea was the reason why he was misunderstood; and this is the reason which has induced me to examine the point more carefully.

In one case of electrostatic induction through the air Faraday replaced a part of the air by sulphur, and found

that the induction was now greater than when the air was there which the sulphur had replaced.

He now compares this case with that of the branching of a current in bad conductors, one of which he replaced by a good conducting wire. Then a stronger current would flow through the wire than flowed through the bad conductor which it had replaced. And as much less electricity would flow through the remaining bad conductors as more through the wire.

This is the meaning of Faraday's words, "Amongst insulating dielectrics some lead away the lines of force from others as the wire will do from worse conductors" (§ 1331).

I shall, in the following, consider some of the applications of this conception of the inductometer as a dividing of electrostatic induction.

Method for Determining Inductive Capacity.

The methods for determining electrical resistance which are based upon division of the current may be transferred to the measurement of inductive capacity. In Wheatstone's bridge, for example, first given by Christie, we determine an unknown resistance as a fourth proportion to three known resistances. For this purpose, the current between a point of higher and a point of lower potential is divided. Two points, one on each of the branches, are found having equal potential. They divide, therefore, the resistances of the two branches in like ratio. Then no current passes round a galvanometer introduced between these points. In the differential inductometer the beginning of the branching is the middle inductric plate, whose electric level is V , and the end of the branching is on the two inductive plates, where the potential is zero. Between the inductric plate and each of the inductive plates let us now introduce a metal intermediate plate of equal potential, so that there are four divisions whose inductive capacities are proportional to one another in pairs. If the inductive capacity of one of these divisions is altered by introducing another dielectric than air, then the capacity of the other division must also be altered, in order to restore the original proportion. The capacity of this second division is changed by altering the distance between its limiting plates until the original

equality of the electrical potential of the two middle plates is again reached. This is recognized by finding that a quadrant electrometer connecting the two shows no deviation. Thus the measurement of inductive capacity is reduced to measuring a length. This method is essentially the same as that recommended by Maxwell and Sir W. Thomson to Mr. Gordon for his measurements of inductive capacity, published a year ago (*Phil. Trans.*).

Theory of the Electrophorus.

A further instance of the branching of induction is found in the electrophorus. Being about to deduce its theory and to discuss the number of electrically effective layers, I shall start from Faraday's differential inductometer. Such a theory of the electrophorus as takes Faraday's views into account has not yet been given. Faraday has not developed it; and the question as to how many layers are electrically effective is not treated by Maxwell, who views the electrophorus not as a symmetrical construction consisting of a plate of ebonite with two movable metallic coverings, but as an unsymmetrical arrangement consisting of a single ebonite plate covered at the back with metal, and having only one movable metal plate.

In this special manner we shall obtain a theory of the electrophorus, and get a fixed point from which to view the different theories based on electrical action at a distance and expressed in the language of the influence theory. We shall approach nearest to that one of these theories which has lately been supported by Herr v. Bezold (*Pogg. Ann.* cxlii.).

The electrophorus, as I view it, is symmetrical. It is a non-conducting plate with two movable coverings, and can thus be used as a Franklin's disk, a Leyden jar, or a condenser. My electrophorus consisted of a circular ebonite plate 1.5 millim. thick and 30 centim. diameter. The coverings were of zinc and were of the same diameter, one of them being provided with an ebonite handle. For other purposes I used three plates of the same shape, of thinner ebonite, together with other plates of zinc of similar dimensions.

The simplest supposition is that the ebonite plate, lying on the one uninsulated metal covering, being beaten by the

fox's tail becomes negatively electric. Then induction starts from this negative layer of this ebonite plate as inductric layer (*inductor*). The inducteous layer (*inducendus*), where the induction ends, is the underlying uninsulated metal plate. After putting on and uninsulating the upper metal plate, however, the induction branches off again from the upper negative layer of the ebonite plate as inductric body (*inductor*), and extends to both the metal plates as inducteous bodies (*inducendus*). If there exists, as we have supposed, only one inductric layer in the electrophorus, namely the negative one on the upper surface of the ebonite plate, the differential inductometer could be viewed as a scheme of the electrophorus. The negative layer of the electrophorus would be represented by the negatively charged middle inductric plate of the differential inductometer; and the two metal coverings of the one apparatus would correspond to the two of the other. Further, this similarity of the two apparatus appears still more complete if we use the differential inductometer as electrophorus for producing electricity. We can uninsulate the one plate of the differential inductometer; and bringing the other near, we can draw out of it a negative spark; then withdrawing it, we can get a positive spark. But one difference now presents itself, and proves the insufficiency of our last scheme. As to the differential inductometer, it is perfectly indifferent whether we uninsulate the right outer plate and insulate and discharge the left one, or if we uninsulate this and change the distance of the right one. We always get positive discharge from that outer plate which we have removed from the negative middle plate. The same kind, namely positive discharge, after removal, must take place if there existed only one inductric layer. Then there is, as Faraday (§ 1255) has already pointed out, no difference as to the kind of induction through air and that through ebonite. Therefore, the ebonite plate being inverted, the upper metal covering (which is now, however, lying on the unrubbed ebonite surface) must give positive electricity when it is lifted, as the plate did before.

This, however, is not the case. If we turn the ebonite plate over, the upper metal covering, on being lifted off, gives negative electricity, while formerly it gave positive.

The differential inductometer, which gave positive in both cases, is therefore not a sufficiently accurate scheme to explain why we get,

- (1) when the rubbed surface is above, a + spark,
- (2) when the rubbed surface is below, a - spark, from the lifted metal.

The simplest completion of the differential inductometer having only one inductor plate, to make a scheme of the electrophorus, is effected by adding a second inductor plate (a positively charged one), which corresponds with the underside of the ebonite plate. In fact, after beating, the ebonite plate attaches itself so closely to the metal sole, that I could hold the sole vertical without the ebonite plate sliding down. At first the negative layer of the rubbed surface induced positive electricity in the metallic sole. The induction is followed by conduction and discharge; a part of the positive electricity goes over from the sole to the lower side of the ebonite plate. On the other side the negative electricity penetrates deeper from the upper surface into the ebonite; so that there are two layers on the ebonite plate—a negative one on the upper side, a positive one on the lower side. For the understanding of the question, it is here sufficient to regard these layers as plane, without discussing the question of the thickness.

With the help of these two layers we have to explain the two observations:—

- (1) The + spark when the metal plate is lifted from the rubbed surface;
- (2) The - spark when it is lifted from the unrubbed one.

For the proof of this theory we shall deduce further consequences from it; and we shall have to confirm these by experiment. The complete scheme of the electrophorus consists of four plates:—

- (1) One negatively charged;
- (2) One positively charged.

These two are as inductive bodies.

- (3 and 4) Both metal coverings as inductive bodies (*inducendus*).

To be brief, we will call the covering on the rubbed negative surface of the ebonite plate "cover;" that on the other, positive,

unrubbed side, "sole;" so that we have in succession
 cover,
 negative layer,
 positive layer,
 sole.

Let us call b the distance of the sole from the positive surface, and d the distance of the cover from the negative layer.

If c is the thickness of the ebonite plate, a layer of air thinner in the ratio of the inductive capacity of the ebonite (2.2) would correspond to it, *i. e.* of the thickness

$$c' = \frac{c}{2.2};$$

so that in our scheme the four metal plates would stand at the distances

$$b, \quad c', \quad d.$$

The rubbed side may contain the quantity of electricity
 $-E$;

and from the sole the part

$$+\alpha E$$

of the induced quantity may have passed on the unrubbed surface:

$$\begin{array}{c|c|c|c} | & b & | & c' & | & d & | \\ \text{Sole.} & +\alpha E. & -E. & \text{Cover.} \end{array}$$

Then, according to the equations (1), (3), and (4), the quantity $-E$ induces a total amount of $+E$, viz.:—

$$\begin{array}{c|c} \text{In the sole.} & \text{In the cover.} \\ \frac{+d}{b+c'+d} E. & \frac{+b+c'}{b+c'+d} E. \end{array}$$

In the same manner the quantity of the lower side $+\alpha E$ induces a total of $-\alpha E$, viz.:—

$$\begin{array}{c|c} \text{In the sole.} & \text{In the cover.} \\ \frac{-\alpha c' - \alpha d}{b+c'+d} E. & \frac{-\alpha b}{b+c'+d} E. \end{array}$$

So that all together is induced:—

$$\begin{array}{c|c} \text{In the sole.} & \text{In the cover.} \\ \frac{+d - \alpha c' - \alpha d}{b+c'+d} & \frac{+b + c' - \alpha b}{b+c'+d} E. \end{array}$$

The numerator of the electric quantity in the cover

$$+b + c' - \alpha b$$

is always positive. That is, there is always induced in the metallic covering on the side of the rubbed surface positive electricity; and we get, on removing the plate, a positive spark.

On the other hand, we recognize that the numerator of the electric quantity in the sole

$$+d - \alpha c' - \alpha d = d(1 - \alpha) - \alpha c',$$

$\alpha c'$ being constant, changes its sign when d increases: viz. d (that is, the distance of the cover) being small, negative electricity is induced in the sole, and we get, on lifting the sole, a negative discharge. This distance d , however, is small, almost nothing, if we invert the ebonite plate and lay it with its rubbed surface close to the metal plate. Then we get from the upper metal plate (the sole), on lifting it, a negative discharge. Thus the two phenomena which we intended to explain, (1) positive discharge in the original position, (2) negative discharge in the inverted position, of the ebonite plate, are deduced from our theory.

The electrophorus contains, therefore, two inductive layers—one negative, on the upper side, one positive, on the lower side of the ebonite plate.

Experiments with the Electrophorus.

If the theory developed by us is true, the consequence which we are about to draw now must be confirmed by experiment. According to the last formula for the numerator of the electric quantity of the sole, positive electricity must be induced in the sole when the distance d of the rubbed surface from the cover is increased, so that we get

$$d \cdot (1 - \alpha) > \alpha c'.$$

This happens indeed, as we see, if we lay the inverted ebonite plate, not immediately on the cover, but place it insulated some height above the cover, as Herr von Bezold has done already. Now the other plate, the sole, being laid on the unrubbed surface, uninsulated and lifted, gives a positive spark. Whilst the sign of the electricity of the cover (the plate on the side of the rubbed surface) is always positive, the sign of the electricity of the sole (the plate on the side of the unrubbed surface) changes. It is positive when the distance of the other metal plate (the cover) from the rubbed layer is small; on the contrary, it is negative when this distance is great.

In other words, when the cover is on the side of the stronger negative inductive layer, the induction of the latter always prevails ; in it positive electricity is induced.

In the sole, however, the nearer but weaker positive layer induces negative electricity. To this there is to be added the positive electricity induced by the stronger but more distant negative inductive layer. The more distant the cover is, the more positive electricity is induced in the sole ; so that, the distance of the cover being large, more positive than negative electricity can be induced.

On the two sides of the ebonite plate the sign of the discharge is differently affected.

1. The ebonite plate lying close to the metal plate, the other, lifted, metal plate gives :—

- (a) The rubbed surface being above, a positive discharge ;
- (b) The unrubbed surface being above, a negative discharge.

2. The ebonite plate lying some height above the metal plate, we get, on the contrary, quite indifferently, whether (a) the rubbed surface is above, or (b) the unrubbed surface, in both cases positive discharges.

Thus we have the means of recognizing not only—

- (1) on which side an ebonite plate is charged positively,
- (2) on which side it is charged negatively, but also
- (3) if it had been charged originally negatively on the one side or

(4) positively on the other side.

For this purpose we place the ebonite plate to be tested close to an uninsulated metal plate. Then we put a second metal plate on the ebonite disk, uninsulate it, and try the electricity induced in it ; the opposite one exists in the upper surface of the ebonite plate.

The ebonite plate is now inverted, put again close to the uninsulated metal plate, and the kind of electricity of the other surface of the ebonite plate is determined in the same manner. Thereby we learn which kind of electricity exists in each surface.

Now the ebonite plate is tested at some height over the metal plate, at first in the original, then in the inverted position ; it is, however, to be lifted so high that in both cases the same kind of electricity is induced. Then the plate contains

a surplus of the opposite kind, and has been charged with it originally on that side on which according to our trial it exists.

Adding the two quantities of electricity which we have found induced in the sole and in the cover, we get, in accordance with our supposition,

$$-E(1-\alpha).$$

The greater the quantity which is induced in the cover, the less is induced in the sole, and *vice versa*. If we join the cover with one electroscope and the sole with another, both electroscopes show a divergence on moving one of the plates, but in opposite senses.

Through a conducting wire from the cover to the sole there flows a current on moving one of the plates; so that the quantity which flows to the sole has left the cover; whilst in the electrophorus itself between sole and cover (*i. e.* in the remaining part of the circuit) changes of induction take place. In the ordinary experiments with the electrophorus this conducting wire is represented by the earth, the base being uninsulated. Here we can speak of a circuit only with the same right as in the case of a telegraphic battery with earth-conduction. If the conduction is broken by insulating the sole after having it uninsulated, the electrophorus is without effect. As to the changes of induction in the interior of the electrophorus (which we may imagine as a polarization of the dielectric), I will further adduce the following experiments:—

If the ebonite plate, put on the uninsulated base, is strongly excited, it attaches itself to the base. If now the cover is put on and both metal plates are uninsulated at the same time, it attaches itself to the cover, so that we can lift the plate by means of the cover. A current of positive electricity has flowed at the moment of uninsulating from the sole through the outer conducting wire to the cover. Before the current has flowed, the induction (polarization) went wholly through the ebonite plate; after the current the inductive (polarizing) effect of the excited layer extends, for the greater part, through the thinner interval of air to the cover, and only for a smaller part of it through the thicker ebonite plate to the sole.

Finally, I may mention an experiment in which branching of induction, as well as branching of conduction, may easily

be observed. I beat the ebonite plate lying on the uninsulated base, and put on the excited surface of the first another equal, but not excited, ebonite plate, and then on this the metal cover. This cover, being uninsulated and lifted, gave positive sparks, just as if there had been in place of the upper ebonite plate a corresponding layer of air. Immediately after this I tried the upper ebonite plate, endeavouring to use it alone as the disk of an electrophorus. But this trial did not succeed; the plate was ineffective.

Now I repeated the experiment, laid the same plate again on the rubbed surface of the first ebonite plate, put on it also again the cover, but left the apparatus unaltered during twelve hours. In this case there is ebonite on the two sides of the inductive layer of the ebonite plate; induction extends from this layer through ebonite to the sole and to the cover. In the same manner the conduction—the penetration or absorption of electricity—must take place to both sides; and I expected that the negative electricity would penetrate not only into the lower ebonite plate, but would go over to the upper plate also. This is indeed what took place. After twelve hours the upper plate could be used as the disk of an electrophorus. Both plates gave now strong and, as it seemed, equal effects. As was to be supposed, the upper plate was negatively charged on its lower surface, with which it lay on the inductive layer, and had therefore to be inverted in order to give similar discharges to those of the first plate.

If we fix our attention specially on the upper ebonite plate, we find:—

(1) That the effect of *induction* through it began almost instantaneously (for immediately after having laid this upper plate on the excited lower one I could put on, lift, and discharge the cover); and that this effect of induction had disappeared also almost instantaneously when I tried to use the plate alone for the electrophorus.

But, further, we have seen (2) that slowly another *inherent* change took place: a penetration of electricity occurred which made the plate more duringly effective as an electrophorus-disk.

XXXII. On a Wave-apparatus for Lecture-Purposes to illustrate Fresnel's Conception of Polarized Light. By C. J. WOODWARD, B.Sc.

[Plate XIV.]

IN the ordinary apparatus for illustrating a plane wave, a series of cranks or eccentrics are attached to one axle, the successive cranks or eccentrics being turned through small, but equal angles. Attached to the cranks or eccentrics are rods, each of which is terminated by a ball, to represent one of the particles of the wave. On turning the axle each ball rises and falls in succession, producing a plane wave. This apparatus is known as that of Powell.

The arrangement I am about to describe consists of two such apparatus, with the rods connected together at right angles to each other. The axles of the apparatus are linked by a rod so that they turn together; and, finally, an adjustment allows the cranks to be placed relatively to each other in any desired position.

The apparatus, divested of supports and accessories, is shown in the figure, drawn in isometric perspective. The cranks 6, 5, 4, 3, &c. turn with the axle X. The cranks are placed successively at 30° angular distance, corresponding in position to the figures on a clock-dial; and as there are twelve cranks, one complete wave is represented by a single turn of the axle.

On the axle X' is arranged a precisely similar set of cranks. Around each crank is put a metal strap connected with a light wooden rod. These rods, a, b, c, &c., a', b', c', &c., are now connected in pairs at their free ends each by a pin; so that each pair now resembles a pair of compasses with the legs at right angles to each other. Around each pin is loosely wrapped a wire having a bullet-cast at one end; and to the other end of the wire, which, from the action of the bullet, projects vertically above the rods, is attached a pearl bead. When each pair of rods has been thus treated, we have a row of beads representing the æthereal molecules composing one wave.

To the axle X is fastened a metal disk, D, to which is attached near the edge a pin, p, surrounded by the loose

handle, H. On the axle X' is a similar disk, but in which eight holes are drilled at 45° from each other. The holes are all tapped, that the pin p' , surrounded by the handle H', may screw into any one of them as desired. A rod, R, connects the two pins p and p' , so that both axles can be turned together.

To describe the action of the apparatus, it should be premised that the starting-point of each crank is when the crank and rod are in the same straight line. Thus, regarding for the moment the first left-hand crank only, it will be at the starting-point, or zero, when the crank 6 has moved on 45° to the left. Similarly with the first right-hand crank, it will be at zero if moved back through 45° to the right; so that, in the position shown in the figure, the left-hand crank has moved through seven eighths of a revolution, while the right-hand crank has moved through one eighth; or the difference of their phases is six eighths, or three quarters; and on turning the cranks, a wave corresponding to circularly-polarized light is produced, each bead describing a circle in succession. Bring now the cranks into the position of the figure, unscrew the pin p' , and, keeping the left-hand crank steady, move the right-hand crank to the vertical position upward. Now the left-hand crank, as before, has made seven eighths of a revolution, but the right-hand crank five eighths, *i. e.* a difference of phase of one quarter. On inserting the pin p' and now turning the cranks, the beads will describe the same wave as before. Now keep the left-hand crank in its first position (the position in the figure), remove the pin p' , and turn the right-hand crank until it is at 45° from the vertical below and to the left. The left-hand crank, as before, has made seven eighths of a revolution, the right-hand one two eighths, or there is a difference of phase of five eighths; and on now inserting the pin and turning the cranks, the wave produced corresponds to an elliptically-polarized ray. Of course, if the difference of phase be made one eighth or any odd number of eighths, the same kind of wave is formed. Again remove the pin, keep the left-hand crank in its first position, and turn the right-hand crank to 45° above the horizontal and to the right. Insert the pin; and on turning the cranks a plane *vertical* wave will be produced, the cranks being in similar positions, each starting at seven eighths of its revolution. Finally, adjust the

right-hand crank till the difference of phase is half a revolution, when a plane *horizontal* wave will be produced on turning the cranks.

I take the opportunity of acknowledging the assistance of my pupil, Mr. William Hall, who made several parts of the apparatus in a simple and ingenious manner.

XXXIII. *On the "Rotational Coefficient" in Nickel and Cobalt.*

By E. H. HALL, *Ph.D., late Assistant in Physics in the Johns Hopkins University, Baltimore**.

THIS article may be considered as the continuation of one published in the 'Philosophical Magazine' for November 1880, under the title "On the new Action of Magnetism on a Permanent Electric Current," in which were given the results of some quantitative investigations of a certain phenomenon recently discovered in the Physical Laboratory of the Johns Hopkins University. It will perhaps be remembered that the essential feature of this phenomenon is the setting up, in a conductor bearing an electric current, of an electromotive force at right angles to the primary electromotive force, when the said conductor is subjected to the action of a magnetic force at right angles to the direction of the current.

In the article alluded to, results were given as obtained with gold, silver, tin, platinum, iron, and nickel. The magnitude of the effect observed, relatively to the strength of the primary current, the intensity of the magnetic field, and the dimensions of the conductor, had not been determined with any accuracy in the case of nickel and tin, though it was known to be comparatively large in nickel and small in tin. The other metals ranged themselves, as regards the numerical magnitude of the effect exhibited, in the following order, viz. iron, silver, gold, platinum—the effect observed in iron being several times greater, and that in platinum several times less, than the effect in gold or silver. The fact of greatest interest, however, was that, if we called the direction of the transverse

* Read May 28, 1881.

effect in iron +, that in the diamagnetic metals, *and in nickel and platinum also*, would be -*.

In view of this remarkable disagreement in behaviour between the two strongly magnetic metals iron and nickel, it seemed highly desirable to make a quantitative investigation of the effect in nickel as soon as possible, and extend the examination to the other strongly magnetic metal cobalt. Most of the experiments to be described in this article relate, therefore, to nickel and cobalt. The examination of the latter was a hasty one, and may well be described first.

No thin strips of the metal being at hand, a slice was sawn from a small block of moderately pure cast cobalt and worked into the form of a cross. To the extremity of each arm of this cross was soldered a thin strip of copper 2 or 3 centim. long, for the purpose of making the electrical connexions. The cross of cobalt with the copper strips attached was now fastened with hard cement to a strip of glass and worked down with a file to sufficient thinness. Before placing the cross upon the glass its thickness, and that of the glass also, was measured by the calipers. After cementing the two together, the total thickness was found, and, again, the thickness of the whole after the cross had been filed down. The thickness of the cross in its final condition was thus estimated at '45 mil-lim., to which value an uncertainty of perhaps 10 or 15 per cent. attaches.

With this apparatus it was found that the direction of the transverse effect in cobalt is +, *i. e.* the same as that in iron.

As to the magnitude of the effect, $\frac{M \times V}{E'}$ was found to be

44×10^{10} , placing cobalt between silver and iron. The specimen of cobalt used, however, contained some nickel (how much is not known accurately); and this doubtless counteracted in part the effect of the cobalt. It seems probable, however, that, allowing for all errors, the transverse effect in

* These signs are given to avoid tedious repetitions. I have here called the effect in iron + simply because its direction in this metal is that which the conductor itself bearing the current would follow, if free to move across the lines of magnetic force under the action of the ordinary "ponderomotive" force. No significance further than this is at present attached to this choice of signs.

cobalt is less than that in iron, other things being equal. The magnetic field used was about 9000 (cm.-gr.-sec.), stronger than has yet been used with iron.

We now return to the consideration of nickel.

The original experiment with this metal had been made with a specimen so irregular, that it had not been possible to determine the magnitude of the transverse effect except in the most general way. The direction had been determined beyond question. The specimen of nickel now employed, and with which the results to be given were obtained, was quite different in appearance and physical condition from the first specimen—though it was obtained in about the same manner, viz. by stripping off a piece of nickel plating from the metal upon which it had been electrolytically deposited. The first specimen was very brittle, the second quite tough. The latter was about .001 centim. thick. As to its purity hardly any thing is known except what is told by its physical characteristics. It is probably affected by all the impurities of ordinary nickel plating. It contains very likely a little cobalt, and perhaps a trace of iron. I understand, moreover, from Professor Wolcott Gibbs, that nickel plating deposited in the usual manner (*i. e.* from an ammoniacal solution) is much affected in its physical properties by nitrogen in some way retained by the metal. It would have been desirable, of course, in all cases to work with pure metals; but such were not at hand, or easily obtainable in the proper form, and it was not thought best to defer the experiments until pure specimens could be obtained*.

The second specimen of nickel showed an effect of the same sign as the first, and numerically greater than the effect which had been observed in the specimens of iron and cobalt used.

It now became a matter of great interest to determine whether the transverse effect had really any connexion with

* This may strike some readers as unwise. It has even been suggested that the difference in behaviour of iron and nickel may be due to impurities in one or the other. This suggestion implies that the transverse effect in these metals is so related to the magnetic properties that, as they resemble each other in one respect, they should also in the other, but at the same time admits that slight impurities, such as would certainly be very far from reversing the magnetic property of either metal, may reverse the transverse effect in the same. This does not seem probable.

the magnetic properties of the metals. It was determined therefore to make a series of experiments, keeping the primary current through the metal as nearly as practicable always of the same strength, but varying within wide limits the intensity of the magnetic field. We should in this way ascertain whether the transverse effect was simply proportional to the strength of the magnetic field, or was related to it in some more complicated manner.

By the term "strength of the magnetic field," as just used, is meant the intensity of the field between the poles which obtains when the metal plate is not in the field. This intensity is measured, as described in the article already alluded to, by withdrawing suddenly from the field a small coil of wire and observing the effect upon a galvanometer in circuit with the coil. This gives what is called the magnetic induction in this part of the field. In general, the magnetic induction in any magnetized space would be changed by introducing into that space a body capable of being magnetized by induction. The well-known expression for the magnetic induction within any such body placed in a magnetic field is (Maxwell's 'Treatise,' vol. ii. art. 428)

$$\mathfrak{B} = \mathfrak{H} + 4\pi\mathfrak{J}; \dots \dots \dots \quad (1)$$

where \mathfrak{H} is the *magnetic force* within the body (Thomson's 'Polar Definition,' reprint, p. 397), and \mathfrak{J} is the *intensity of magnetization* (Maxwell, art. 384).

Now, in case of uniform magnetization, \mathfrak{H} is equal to the intensity of the field as it would exist if the body magnetized by induction were removed (*i. e.* just what we measure by means of the coil and galvanometer), together with the force exerted by what we may call the magnetism induced on the surface of the magnetized body. This latter force will, of course, depend upon the shape and dimensions of the body. If it is a very thin disk, the reaction of the induced magnetism will, as Maxwell remarks, be equal to $-4\pi\mathfrak{J}$; and in this case, writing \mathfrak{F}^* for the intensity of the magnetic field as above defined, we have

$$\mathfrak{H} = \mathfrak{F}^* - 4\pi\mathfrak{J} \quad \dots \dots \dots \quad (2)$$

Substituting in (1), we have

$$\mathfrak{B} = \mathfrak{F}, \quad \dots \dots \dots \quad (3)$$

* Called M in previous article.

which means that, in a very thin disk magnetized by induction, the magnetic induction is just what it would be in the space occupied by the disk if the disk were removed from the field. Now the strip of nickel which we employ has a width 600 or 800 times its thickness ; and it has been assumed that we may, for our present purpose, regard it as such an infinitely thin disk as Maxwell supposes. The error resulting from this assumption may easily be seen to be small. At the centre of the strip of nickel the real value of \mathfrak{B} would be perhaps $\frac{1}{10}$ of one per cent. greater than the value as above determined. At a point 1 millim. from the edge of the strip the error might amount to $\frac{1}{3}$ or $\frac{1}{4}$ of one per cent.; while at $\frac{1}{10}$ millim. from the edge it would perhaps be two or three per cent. The average of the real values of \mathfrak{B} , therefore, at points along the line running across the strip from one side connexion to the other, is probably a rather small fraction of one per cent. greater than the value obtained on the assumption that \mathfrak{B} is equal to \mathfrak{F} . This error is, to be sure, not constant; but it is nearly so up to $\mathfrak{B} =$ about 5000; and when it begins to change rapidly, it grows smaller.

Its influence upon the curve given further on must be very small. We assume therefore, as stated above, that by determining the strength of the magnetic field by means of the coil and galvanometer before the nickel is placed in the field, we ascertain with sufficient accuracy the value of the magnetic induction in the nickel strip itself when placed in the magnetic field. The advantage of determining this quantity is of course very great ; for though we are probably unable to say what is the exact physical nature of magnetic induction, we do attach to the quantity represented by that term a very definite and important mathematical significance.

It was designed, therefore, to investigate the law of the variation of the transverse effect with the variation of the magnetic induction. Nickel was the best metal to experiment upon, for the following reasons: the strip of this metal at hand was very thin ; the transverse effect appears to be essentially more powerful in nickel than in iron or cobalt ; the magnetic permeability of nickel changes more rapidly than that of iron or cobalt with high magnetizing-powers.

As it was desired to determine simply what function of the

magnetization the transverse effect would prove to be, the primary current through the nickel strip has been kept approximately constant, the greatest variation from the mean being probably not many per cent., as will be shown further on. Within these limits it has been assumed that the transverse effect may be considered a linear function of the direct current.

It should be here stated that this latter relation has not yet been proved to hold rigidly even in a non-magnetic conductor like gold; and the matter must some time be investigated, though there seems to be no reason to think that the assumption, as above limited, can prove to have involved any considerable error.

The intensity of the magnetic field, and so the magnetic induction in the nickel plate, has been varied from about 1600 to about 10,000 in absolute (cm.-grm.-sec.) measure.

In the course of this investigation I have become indebted to nearly every one connected with the Physical Laboratory of the Johns Hopkins University, but particularly to Mr. S. H. Freeman, Fellow in Physics, and Mr. H. R. Goodnow, Special Student in Physics, who for a while carried on the experiments together. Mr. Freeman especially worked with me for a long time; and several suggestions of his in regard to the arrangement of apparatus and the method of experimenting were adopted with great advantage to the work.

In my last article on this subject the results of measurements were given in the form $\frac{M \times V}{E'}$, where M was the strength of the magnetic field*, V was the direct current divided by the section of the conductor, and E' was the transverse electromotive force per centimetre of the width of the strip. In that article were given certain reasons for thinking the above quantity more likely to be a constant for any given metal than the quantity $\frac{M \times E}{E'}$, where E is the electromotive force per centimetre of the *length* of the metal strip. Recent developments, to be spoken of further on, raise the question whether the ratio $\frac{M \times E}{E'}$ will not after all prove to be the more fundamental and invariable quantity; but as E is rather dif-

* Called \mathfrak{F} in this article.

ficult to determine with accuracy, and as in any given strip of metal V is likely to remain under ordinary conditions of temperature &c. very nearly proportional to E , the use of the former quantity will be retained for this article at least. The values of M [\mathfrak{F}] will be given separately, however; and, for convenience in plotting the results, the quantity $\frac{E'}{V}$ will be used instead of $\frac{V}{E'}$. The values of \mathfrak{F} will, in plotting, be laid

off as abscissas, and the values of $\frac{E'}{V}$ be taken as ordinates.

This method of plotting gives a simple curve in the present case, and puts the results of the experiments in form to be compared with those of previous investigations of some of the

magnetic properties of nickel. It is this quantity $\frac{E'}{V}$ which,

after Maxwell, in accordance with the suggestion of Mr. Hopkinson*, is now called the "rotational coefficient" of nickel.

Mr. Hopkinson has suggested "rotational coefficient of resistance;" and possibly some quantity might be found which

would demand that title. At first sight $\frac{E'}{V}$, which is an elec-

tromotive force divided by a quantity proportional to a current, would seem to be of the nature of a resistance; but it is to be noticed that the electromotive force E' is not the cause, but the effect, of the current implied in V .

In the experiments which I have described in previous papers, no account was taken of the temperature of the conductor experimented upon. When these experiments upon nickel, however, had been going on for a long time, it began to be suspected that the temperature of the room, and so of the nickel plate, did exercise a very considerable influence upon the magnitude of the transverse effect as expressed by the ratio $\frac{E'}{V}$.

* Phil. Mag. Dec. 1880, p. 430. Prof. Rowland has (Phil. Mag. April 1881, p. 254) remarked upon Mr. Hopkinson's note. Maxwell did not know any such effect to exist. In fact he expressly stated that it probably did not exist; yet, seeing the possibility of it, he let fall the phrase which seems now best fitted to define this newly discovered property of the metals.

A few hasty experiments with considerable ranges of temperature in the room indicated very decidedly that the temperature was a factor to be considered, and that the higher the temperature the greater the value of E' , other things being equal. The magnitude of this influence can hardly be determined from results thus far reached. It may prove that the transverse electromotive force E' is no more increased by a rise of temperature than the direct electromotive force E^* is; and in this case it would appear, as intimated above, that the ratio $\frac{E'}{E}$ is the one to be investigated rather than $\frac{E'}{V}$.

Future investigation must determine this matter; and meanwhile it has been sought to avoid evil consequences by regulating, as well as practicable, the temperature of the nickel plate. Sometimes an experiment had to be made at a rather high temperature for instance; and an attempt would then be made to balance this by making another with about the same strength of magnetic field but at a low temperature, or *vice versa*. There was, however, even now no attempt to determine the actual temperature of the nickel; but a thermometer was hung up with its bulb close to the plate, and as nearly as practicable always in the same position with respect to the latter, and both plate and thermometer were protected from sudden changes of temperature. As the nickel was of course heated by the current, its temperature must have been always considerably higher than that indicated by the thermometer. Moreover this difference must have varied somewhat with the strength of the direct current; so that the temperature read can be assumed to give only a very rough indication of the changes in temperature of the nickel.

None of the numerical results of measurements made with nickel before the disturbing influence of temperature was discovered are here published. In some of the results afterwards obtained, however, the effects of variations of temperature can apparently be detected, as will be pointed out hereafter.

The general method of experiment has been already sufficiently described in previous papers. There will now be given

* Apparently E' is in nickel affected by temperature more than E in most metals would be; but the rate of increase of the resistance of nickel with rise of temperature seems not to be known.

in tabular form the most important data involved in this examination of nickel, and the values of $\frac{E'}{V}$ obtained. The absolute strength of the primary current through the nickel strip in any case is not given, as, by the method of experiment, both the constant of the galvanometer used to measure this current and the horizontal intensity of the earth's magnetism at this galvanometer (this intensity being assumed to be constant during any one determination of $\frac{E'}{V}$) are eliminated from the formula for $\frac{E'}{V}$. There will be given, however, the tangents of the angles of deflection of the galvanometer-needle, in order to show about what were the limits of variation of the primary current. It may be well to state that this current was what one Bunsen cell would send through—say, six or eight ohms. It will be seen that there are variations of about 6 per cent. in $\tan \alpha$; and the actual variations in the primary current may possibly have been considerably greater than this; for on March 11th, 12th, and 14th the galvanometer stood in a different room from that in which it was placed for the previous observations, and the horizontal intensity of the earth's magnetism was probably somewhat different in the two places. I have, however, as stated above, assumed that within the limits of these variations the value of E' is a linear function of the direct current. It is evident that no large error can result from this assumption.

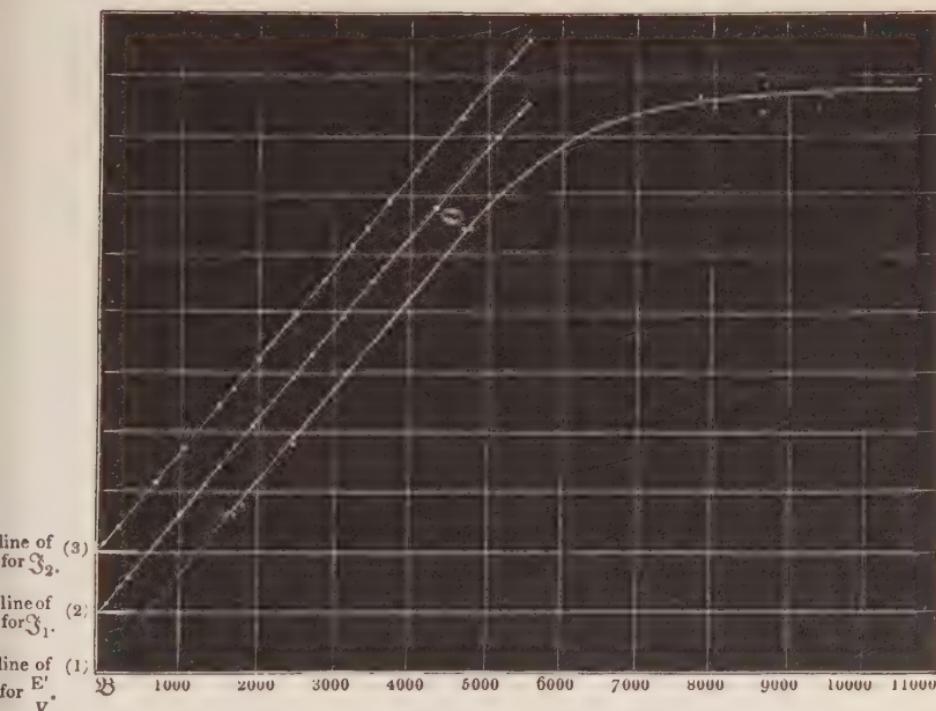
It will be seen from the table that the experiments began with the smallest values of the magnetizing force and went on by stages to the highest. This is the proper course to follow in order to avoid at any stage of the magnetizing force the effects of a previous stronger magnetizing force. It must, however, be stated that, before the series of experiments whose results are here published was begun, the nickel had already been several times subjected to a magnetizing force of about 7500, *i. e.* four or five times as great as the forces with which this series begins. The question, of course, arises whether there may not have been induced by this means a permanent magnetism sufficient to affect the results of subsequent experiments. In order to settle this question as far as possible, a small piece of nickel film, of the same quality as the strip in

use, was first subjected to the action of a field of about 7000 or 8000. It was then placed in a field of perhaps 1500 or 1600, whose direction was such as to tend to reverse any permanent magnetization which might have been induced in the film by the previous field. It was found that now in the second field the nickel became magnetized, temporarily at least, in the direction of that field. No attempt, I believe, was made in any case to detect the permanent magnetization. In this trial the small piece of nickel film was magnetized, not in the direction of its thickness, but in a lateral or longitudinal direction; so that we do not here have an exact parallel to the case of the strip; but it seems probable that magnetization in the direction of the shortest dimension would be much more easily disturbed than that in a longitudinal direction. Moreover, just before the series of experiments was begun whose results are here published, quite a long series was made with magnetizing forces about equal to those with which the published series begins; and this treatment would have tended, no doubt, to obliterate any traces of permanent magnetism due to the action of previous higher forces, even if this permanent magnetism had been much greater than we have any reason to suppose it was. On the whole, therefore, the probability of any considerable error from this source seems to be very small.

Date.	Temperature.	Tan α .	\mathfrak{F} or \mathfrak{B} .	$\frac{E'}{V} \times 10^{10}$.
Feb. 24, 1881.....	18.5	.330	1667	209.3
25, ".....	22.0	.332	1655	211.1
26, ".....	21.5	.335	1664	208.1
26, ".....	16.0	.336	1735	213.2
28, ".....	19.5	.333	2512	314.3
28, ".....	20.0	.333	2512	307.0
Mar. 1, ".....	20.0	.330	4734	596.1
2, ".....	19.5	.327	4775	596.4
5, ".....	19.0	.338	6540	735.5
7, ".....	20.0	.339	6415	726.7
7, ".....	20.5	.340	7996	761.0
10, ".....	21.5	.324	7791	771.0
11, ".....	21.0	.342	8712	783.5
11, ".....	18.5	.343	8644	755.1
12, ".....	20.0	.338	9561	772.4
12, ".....	18.0	.338	9708	759.8
14, ".....	21.0	.326	10720	793.3
14, ".....	21.0	.323	10290	785.6

Gal. in
new
position.

Laying off the values of \mathfrak{B} on the base-line, and taking the values of $\frac{E'}{V}$, plotted on a convenient scale, as ordinates, we have curve (1). It will be seen that this curve is nearly straight for a considerable distance, and that if this portion were extended backward it would pass very near the origin. Between the points corresponding to $\mathfrak{B}=5000$ and $\mathfrak{B}=8000$ the line tends strongly to the right, and thenceforward it continues as if asymptotic to some horizontal line not very far above.



The points marking the highest values of $\frac{E'}{V}$ do not fall so well in line as one might wish ; but by looking at the table it will be seen that there were considerable variations of temperature accompanying these observations ; and to these variations the irregularities can perhaps be in some part attributed.

We see now at once from the diagram that $\frac{E'}{V}$ is not proportional to \mathfrak{B} , the magnetic induction in the nickel. Can we find any magnetic quantity to which it is more simply related ?

If we turn to the observations of Prof. Rowland on nickel*,

* Phil. Mag. Aug. 1873 and Nov. 1874.

we find that they can, as he says, be plotted in several ways. In order to compare them with the observations above given, we need to plot them in some manner that will lay off the values of \mathfrak{B} (M in Prof. Rowland's first paper) on the base-line. We may then take as ordinates the values of the magnetic permeability, as Rowland has done in his first paper (plate iii.), or the values of κ , Neumann's coefficient of induced magnetization, or the values of \mathfrak{H} , the "magnetic force" * within the nickel, which would be a reversal of one method used by Rowland in his first paper (plate ii.); or, finally, we may use the values of \mathfrak{J} , "the intensity of magnetization according to the German theory," as Rowland calls it in his second article.

Having plotted these various curves we may compare them with (1) above, in order to determine whether our quantity $\frac{E'}{V}$ corresponds most nearly to μ , to κ , to \mathfrak{H} , or to \mathfrak{J} .

The curve for $\mu \left[\frac{\mathfrak{B}}{\mathfrak{H}} \right]$ will, long before \mathfrak{B} has reached the higher values used in the curve for $\frac{E'}{V}$, have reached a maximum and returned nearly to the base-line. The curve for $\kappa \left[\frac{\mathfrak{J}}{\mathfrak{H}} \right]$ will be very similar to that for μ . We do not, then, find suggested a close connexion between μ or κ and the quantity we are studying.

The curve for \mathfrak{H} bends *upward*, and is therefore quite dissimilar to that for $\frac{E'}{V}$.

The values of $\mathfrak{J} \left[\mathfrak{B} \frac{\kappa}{\mu} \right]$ obtained from two of Rowland's series†, made either with different specimens of nickel or with one specimen under quite varied conditions, give the curves (2) and (3). A separate base-line is taken for each of the three curves; and the ordinates of (2) and (3) have been plotted on different scales, in order to make the general inclination of those curves agree with that of (1). The values of \mathfrak{B} , however (and this is the essential particular in the plotting), are given on the same scale for all three curves. The important facts about

* Thomson's 'Polar Definition,' reprint, p. 397; and Maxwell's Treatise, art. 393.

† Phil. Mag. Aug. 1873, p. 153, and Nov. 1874, p. 327.

the lines (2) and (3) are that they are sensibly straight for a long distance, that they appear to come nearly straight from the origin, and that they begin to bend perceptibly toward the horizontal when B becomes 4000 or 5000. Although these lines are carried only a short distance beyond this region, we can yet be sure that the bend is not due to faulty observations; for to make an error of 1 per cent. in the value of \mathfrak{J} at this point would require an error of very many per cent., say 20 or 30, in the value of μ as determined by Prof. Rowland. From the manner and rate at which μ was changing at the points where his experiments ceased, it seems almost certain that these lines would continue to bend, and for a time to bend rapidly. Indeed the curve in which Prof. Rowland has continued μ beyond the range of his experiments would indicate that the curves (2) and (3), if continued a short distance further, would turn downward and approach the baseline. This, however, would mean that the magnetization \mathfrak{J} actually *decreases* after a certain point with *increase* of the magnetizing-force. The possibility of this is spoken of by Rowland*; but there seems to be no experimental evidence of such an effect; and if it does not exist, it appears altogether probable that the lines (2) and (3) would become asymptotic to horizontal lines lying considerably higher than any points reached by the curves as here given.

We can therefore say that, so far as actual experiments have gone, there seems to be much tending to prove a very simple and intimate relation in nickel between the transverse effect and the "magnetization according to the German theory."

It would, of course, be desirable to test for some more minute agreement than has yet been traced between the curves for $\frac{E'}{V}$ and \mathfrak{J} ; but such a testing would probably be difficult to make. An exact agreement could not be expected; for it would probably be almost impossible to obtain exactly the same quality and condition of metal in the very different shapes required for experiments on \mathfrak{J} and experiments on $\frac{E'}{V}$.

There are, however, certain minute characteristics which would belong to all curves for \mathfrak{J} . Thus (2) and (3) should not

* Phil. Mag. Nov. 1874, p. 322.

be straight at any point. They are lines of double curvature, the steepest part of each being not far from $B=2000$. The curvature in this region, however, is very slight; and to detect a corresponding curvature in the line for $\frac{E'}{V}$, if such exists, would be a matter of considerable difficulty, though not, perhaps, impossible.

Having gone thus far with nickel, we might, were it not for the anomaly presented by the sign of the rotational coefficient in iron and cobalt, make a prediction by analogy as to what would prove to be the character of the curves for $\frac{E'}{V}$ in these metals. We should say that they would be sensibly straight for a much longer distance than the curve for nickel, and that in fact it might be difficult to carry the magnetization far enough to detect any marked departure from a straight course. So great a difference in behaviour as is indicated by a reversal of the sense of the transverse effect, however, makes any such predictions hazardous.

This difference of sign in the rotational coefficients of the magnetic metals is so anomalous and so important a fact, that one returns again and again to its consideration. Quite recently the determination of this sign for all three metals has been made anew. I have now tested, in all, four plates of iron (three of them having been cut from the same sheet, but the fourth being of a different thickness and probably of a somewhat different character), two plates of nickel (certainly very different from each other in condition), and one specimen of cobalt. With all these the record is perfectly consistent. Nevertheless it would be desirable to examine more specimens, and those differing widely in character. Different experimenters have observed many peculiar effects in iron under the influence of magnetism and the electric current, magnetism and mechanical strain, or the combined influence, which in a certain form we have here, of all three; and these effects appear to differ greatly, and sometimes to be of different signs, in soft iron and hard iron or steel. Thomson has found* that, under conditions of the above character, soft iron and nickel are, in certain apparently very important particulars,

* Phil. Trans. May 1878.

opposed in behaviour. I have looked in vain through all the facts of this kind with which I am acquainted for any plausible explanation of the fundamental phenomenon of the transverse action, nor can it be said that any clue has been found to the cause of the diversity observed. Nevertheless the opposition which Thomson has found in the behaviour of soft iron and nickel, under conditions of magnetism and mechanical strain, furnishes an analogy which should not be lost sight of. Thomson has moreover noticed that the effect which he was studying in soft iron became reversed in this metal at a very moderate value of the magnetizing force. It might be well to test the direction of the transverse effect also with very small intensities of the magnetic field.

An extended examination of the effect in iron and cobalt, similar to that which has been made in the case of nickel, should be undertaken as soon as practicable. It will require very great intensities of the magnetic field and a very large battery* to carry these metals through a range of magnetiza-

* In the experiments here detailed, the largest battery used has consisted of 48 large Bunsen cells arranged 8 in series. The resistance of each cell was probably something more than an ohm; the resistance of the electromagnet is, I think, rather less than an ohm. The resistance of the connexions was considerable, however; and the battery probably gave about its best effect.

After this powerful battery had been applied to the electromagnet, a rather singular effect was observed on returning to the use of weak currents. In making observations in the usual way to determine the strength of the field produced by these weak currents, it was found that the impulses given to the galvanometer-needle were very capricious. These observations may be arranged under two heads, + and -, according to the direction of the current through the electromagnet, this current being usually reversed after each withdrawal of the little coil from between the poles. The observations being arranged in this way, it would be found that there were occasional sudden changes of many per cent. in the readings in the same column. Of course the most obvious explanation of the phenomenon was that some connexion was loose, either in the circuit of the galvanometer and the test-coil or in that of the electromagnet. That the fault was not in the former circuit was made probable by the fact that by means of the earth-inductor, which was in the same circuit, quite uniform deflections of the galvanometer-needle were produced. To test for a fault in the magnet circuit, a tangent-galvanometer was introduced into it and its deflections observed during the series of observations on the strength of the field.

The readings of the tangent-galvanometer decreased slowly with the

tion corresponding to that through which nickel has been examined. It seems doubtful whether the magneto-electric machine can be here employed, as the current which it produces may not be sufficiently uniform to be used with advantage.

The examination of the non-magnetic metals also should be continued as fast as circumstances will permit, with the object of determining the sign and, when practicable, approximately the magnitude of the rotational coefficient in every case.

In my article of last November I stated that, in accordance with Prof. Rowland's suggestion, I had tested the Kerr effect with one specimen of nickel, and found it to be of the same sign as the effect which Kerr had observed with iron. In order to prevent mistakes, the experiment was repeated with iron, or rather, I suppose, with steel, the result being the same which Kerr had obtained. The surface of nickel first used in this way was the coating upon one of the plates of Prof. Rowland's absolute electrometer, the metal beneath being brass. Two other specimens of nickel have since been tried. One was a coating deposited electrolytically directly upon the iron pole of the electromagnet, the other was a nickel film fastened with soft cement to a plate of glass. Probably none of these specimens was pure; but (and this is a matter of more importance) the third was of precisely the same character and origin as the specimen in which the transverse effect was studied. The Kerr effect is of the same sign in all three plates of nickel, *i. e.* of the same sign as the effect in iron.

One specimen of cobalt has also been tested for this effect.

running-down of the current; but the changes were quite regular, and not at all of a character to account for the irregularity of the other observations. The most plausible explanation I could finally propose was, that this irregularity in the strength of the magnetic field was due to a sort of uncertain struggle between the action of the present weak magnetizing current, and the magnetization previously induced by the strong currents in the poles of the electromagnet, which are not, I believe, of very soft iron, and are probably capable of considerable permanent magnetization. I do not by any means feel able to assert, from my rather hasty observations, that there can be no other explanation. I have, however, simply thought the matter of sufficient importance to justify me in recording what seemed to be the fact.

A block of cast cobalt, quite similar to that from which was cut the cross mentioned in the first part of this article, was sawn in two, and one of the fresh surfaces was made quite smooth with a file and then polished with emery. It is not difficult to get a sufficiently good surface. An hour's work might prepare it.

With sunlight and a tolerably strong magnetic field, say 4000 (cm.-grm.-sec.), the rotation produced by cobalt was detected, and found to be of the same sign as that observed with nickel and iron.

The fact that nickel behaves like the other magnetic metals in optical effect, but differently from them in the transverse electrical effect, is on its face undoubtedly an argument against the theory which refers the two effects to the same cause. In order if possible to examine the optical effect in a somewhat different manner, an attempt has been made to detect an action of magnetized nickel upon polarized light transmitted directly through it. For this purpose a thin piece of glass was coated over a part of its surface with nickel by Wright's process*, the action being stopped before the nickel film became thick enough to be opaque. It was found, however, that the glass alone, although only about $\frac{1}{6}$ millim. thick, perceptibly rotated the plane of polarization of the light sent through it when subjected to the very strong magnetic field employed. The action produced by the nickel and glass together was of the same sign as that produced by the glass alone; and as the magnitude of the effect could not in either case be measured with any accuracy, the experiment was quite negative in result. I now, however, took a glass tube, fused the end, and blew out the bubble till it burst. A piece of the exceedingly thin film thus obtained was subjected to the action of the magnet, and most strenuous endeavours were made to detect its action upon the beam of polarized light. This action must have been exceedingly slight, though there is some evidence, which it is not necessary to give, that it was detected. The glass, however, was coated as the first piece had been, and again with its coating subjected to the action of the magnet. The trial was, for certain reasons, rather unsatisfactory; and although no rotation of the plane of polarization was now detected, I do

* Amer. Journ. of Science, Jan. 1877, p. 49, and Sept. 1877, p. 169.

not think this fact can be taken as evidence that the effect of the nickel had counterbalanced the effect of the glass. Both these experiments on direct transmission have been, we may say, quite negative; but these details are given as marking out a line of research which will probably be some time resumed.

An endeavour has also been made to detect a possible rotational effect due to reflection from silver when strongly magnetized*. For this purpose two strips of silver upon glass were used; and these strips were fastened one upon each pole of the magnet, the silvered surfaces being turned toward each other and as nearly parallel as practicable. The poles being brought near together and the light being let in between the silvered surfaces at a large incidence, it was possible to obtain twenty or thirty successive reflections before the beam emerged toward the analyzing Nicol. Certain difficulties were introduced by this arrangement; but in spite of these I think that, if the action of silver had been one tenth as strong as that of iron, the effect would have been detected. No such effect was observed.

XXXIV. On the Results obtained from a Modification of Bunsen's Calorimeter. By Prof. B. STEWART and W. STROUD†.

A DESCRIPTION of this instrument was brought before this Society on June 26, 1880, and afterwards appeared in the Proceedings of the Physical Society, vol. iv. p. 52, and Philosophical Magazine, vol. x. p. 171. The results obtained at that time were not very good, owing greatly to the large bore of the capillary tube, which necessitated the use of a large quantity of substance, the consequence being that the mercury in the bulb was so much heated that the loss of heat by radiation was not inappreciable during the time of performing the experiment.

Since then a new instrument has been obtained from Mr. Casella, having a much finer capillary stem with an empirical scale engraved on it. By means of it very good results have

* In this experiment I had the very efficient assistance of Mr. Arthur W. Wheeler, Fellow in Physics, whose untimely death is so deeply lamented.

† Read June 26, 1881.

been obtained with very small quantities of substances ; in fact, it has been found (within certain limits) that the smaller the mass of the substance the better.

At first we endeavoured to make allowance for the loss of heat during the experiment by constructing a curve of cooling, and adding to the rise obtained by dropping in the substance the amount that had been lost while the experiment was being performed, according to the curve of cooling. This method, however, did not yield good results.

Our final method of operating is comparative only ; that is to say, one metal, whose specific heat is accurately known, is chosen as the standard to which all other substances are referred, and, in order that the conditions may be the same in all cases, we first ascertain the rise obtained by introducing a given weight of mercury at a given temperature (that of the air), and when the instrument has cooled throw in the substance whose specific heat is desired. During the two experiments the temperature of the air is usually nearly constant ; and in that case no correction for temperature is required.

A second point to be noted, is to put in such a weight of substance as will produce about the same rise as was obtained with the mercury. It is thus possible to make the determination of the relative specific heats under almost precisely the same circumstances, a condition which is eminently conducive to good results.

In practical working with the instrument the following hints may prove useful :—

1. The test-tube is always filled with water to the same height before being cooled, this height being that of the level of the copper or slightly above it.

2. About 6 grm. of mercury is placed at the bottom of the tube, unless the substance to be experimented on is attacked by mercury. The object of this is to allow the solids that are introduced to part with their heat more freely. The substance comes thus into much more intimate heat-contact with the mercury of the thermometer than it would if it rested upon the glass alone. If, for instance, some pieces of sulphur are allowed to fall to the bottom of the tube, they sink partly into the mercury, and quickly give up their heat to it, so

that it is more nearly the same as if the mercury had been heated.

3. A large-sized box is employed, in which plenty of ice is placed, so as to completely surround the instrument.

4. As the mercury sometimes moves very sluggishly, gentle tapping, not vertically, but horizontally, has been found very beneficial.

We will now take an example to illustrate the method of working :—0.396 grm. of mercury was weighed out and allowed to remain for some time in contact with the bulb of a thermometer. Meantime the instrument was gently tapped and the height read. The mercury was then poured in after the thermometer-reading ($18^{\circ}6$ C.) had been taken. The rise obtained on the scale was 15. An experiment with iron was then made ; and since the specific heat of iron is about four times as great as that of mercury, .144 grm., or about one fourth the weight of the mercury, was taken. The temperature was still $18^{\circ}6$; and the maximum rise obtained was 18 ;

$$\therefore \text{specific heat of iron} = 0.333 \times \frac{18}{15} \times \frac{396}{144} = 0.110.$$

The following results have been obtained:—

Substance.	Weight.	Temper- ature.	Rise.	Specific heat. Hg = .0333.
{ Mercury	·364	16·8	15	
Sulphur	·148	17·1	32	·174
{ Mercury	·325	16·2	10	
Iron.....	·144	16·3	15	·110
Sulphur	·134	16·2	22	·178
{ Mercury	·396	18·6	15	
Iron	·144	18·6	18	·110
Sulphur	·081	18·6	16	·174
Mercury	·400	18·0	14·5	
Iron.....	·144	18·0	18·0	·115
Sulphur	·083	18·0	15·5	·175
Mercury	·505	15·8	14·5	
Iron.....	·145	16·2	13·5	·112
Sulphur	·118	16·2	17·0	·177